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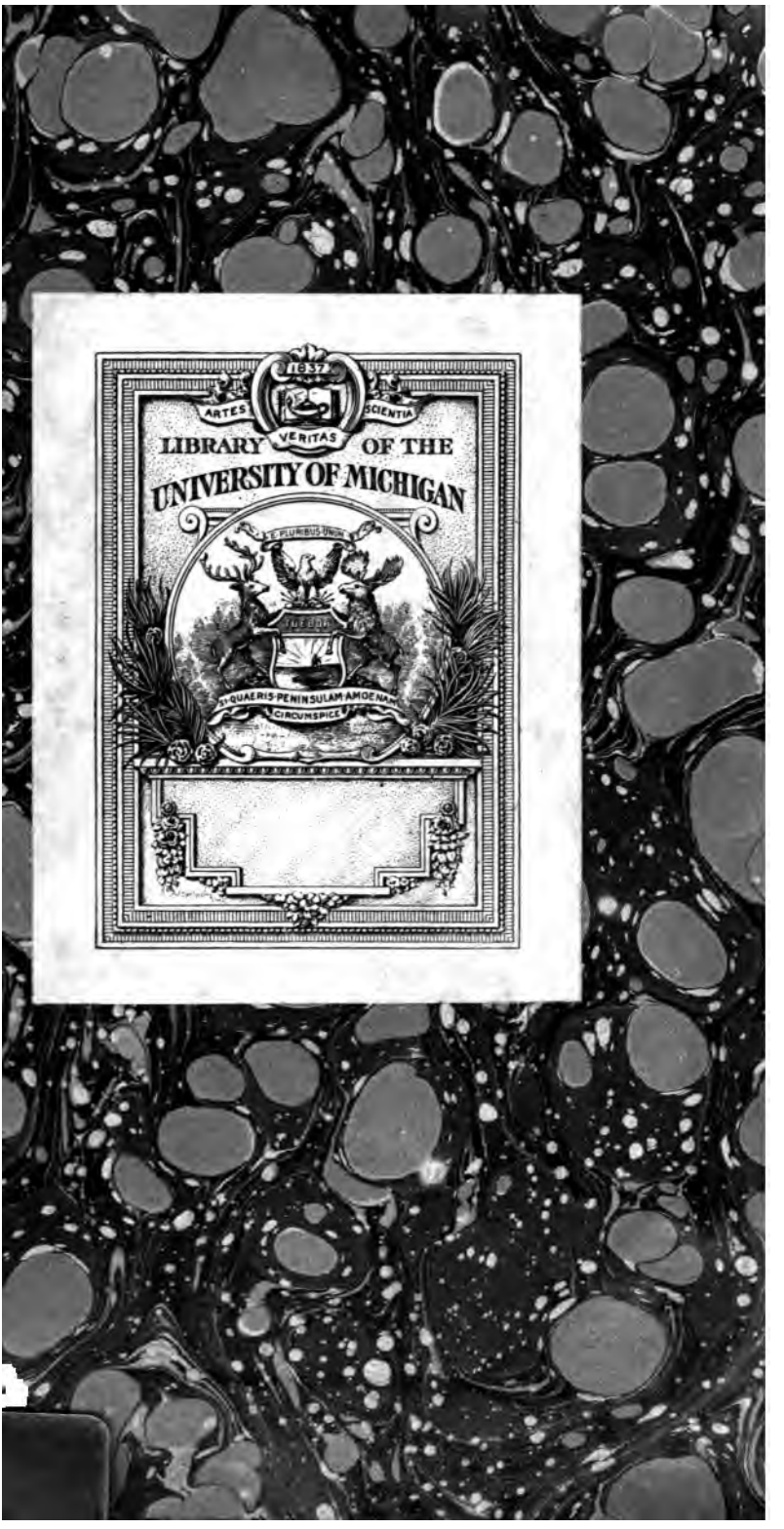
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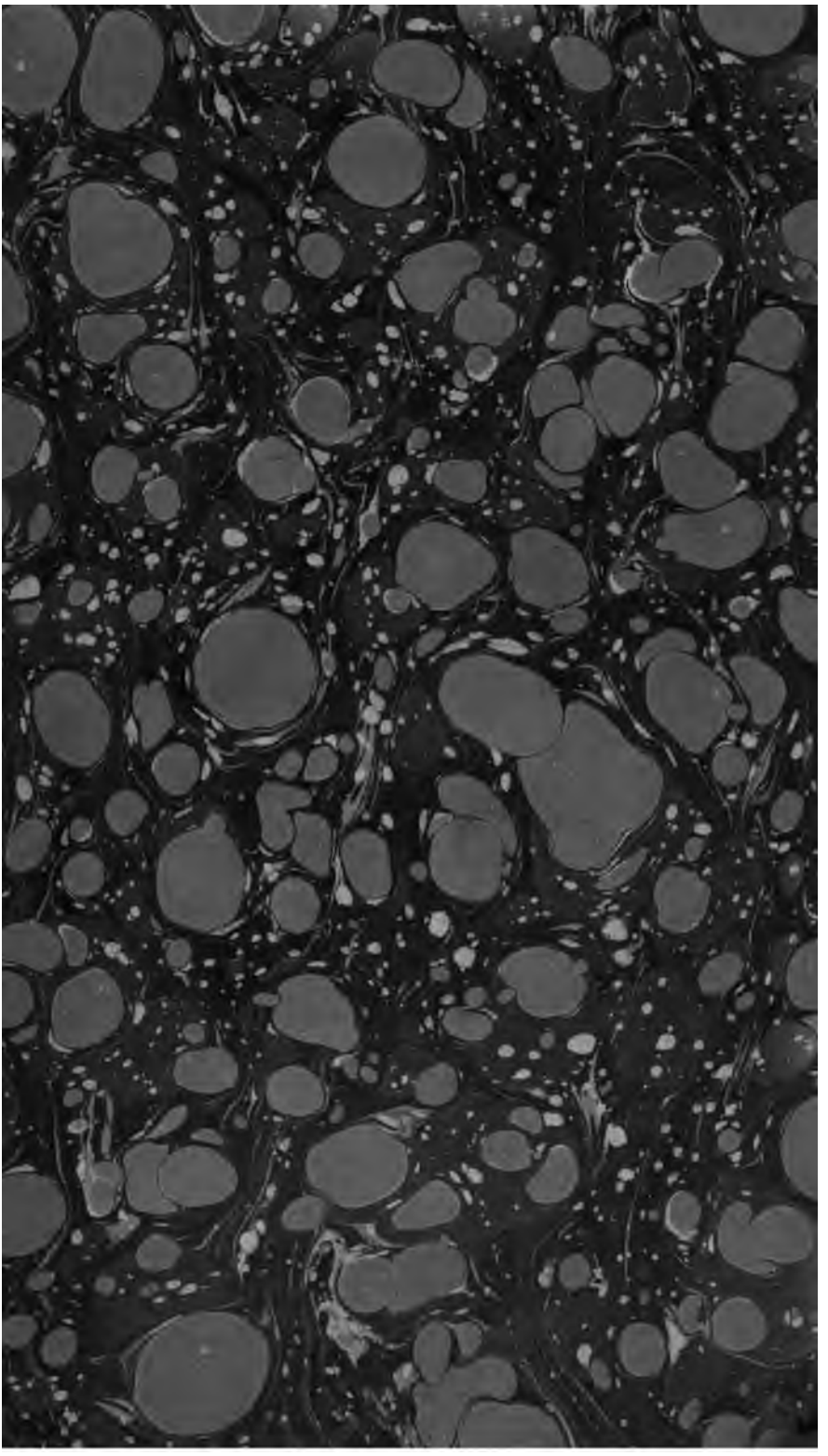

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specific difference between the constitution of steam at a high temperature, and that produced at the common boiling point. Every body knows, that water, in the open air, boils at 212 degrees of Fahrenheit; but in a Papin's digester, or closed vessel, or under the pressure of a column of mercury, it may be heated very considerably above 212° without boiling at all. In the exhausted receiver of an air-pump it boils much below 212°, the boiling point in this and all the other cases depending upon the superincumbent pressure.—So that water always begins to boil in the open air when the elasticity or force of the steam becomes equal, or a little more than equal, to the pressure of the atmosphere. The force of steam, therefore, of the temperature of 212°, is equal to the weight of the atmosphere, or to a column of mercury of about 30 inches high. But this force is very rapidly increased by increasing the temperature;—so that if steam, in contact with water, is heated to about 40° above the boiling point, the force is doubled, and it becomes capable of sustaining a column of mercury of 60 inches: and if the temperature is raised about 55° still higher, or to 307°, the steam is then equal to four atmospheres, or 120 inches of mercury. I make these statements from the table that

accompanies Mr. Dalton's interesting Paper on this subject, in the 5th vol. of the Society's Memoirs. In a valuable Essay on the article "Steam," published in the 17th vol. of the Encyclopædia Brit. and in the account of Mr. Betancourt's experiments on the same subject, the force of steam over water is stated to increase in a still greater ratio with respect to the temperature; but I rely on Mr. Dalton's table, which seems to have been constructed with great accuracy. When I first began to consider this subject, the question that naturally occurred to me, was, How does this comparatively small addition of temperature produce so remarkable an increase of force? I could find no satisfactory answer to this question in the papers that had been published; nor did the information I obtained from such of my friends, as were most conversant with the subject, supply the defect. I was told, that Mr. Watt had made experiments; but could not learn the nature of them, or the precise results. It was supposed, however, that, as steam over water increased in temperature, it continued to combine with a greater quantity of heat than what was indicated by the mere increase of temperature;—so that steam of double the force of the atmosphere probably contained twice as much heat, in the same bulk, as

steam at the common boiling temperature. I take for granted it will be remembered, that the steam which rises from boiling water indicates the same degree of heat to the thermometer as the water does, viz. 212°; but that it, in fact, contains about 940° of latent heat, not appreciable by the thermometer;—so, that if steam, which is double the force of the atmosphere, had combined with a proportionate quantity of heat, it must necessarily contain upwards of 1800° of heat more than what would be indicated by the thermometer, at least, if we consider the latter steam of the same density as the former.

I proceed to an explanation of the experiments.

First Class of Experiments.

I procured a small oblong cast-iron boiler, capable of holding about two gallons of water, into which was fitted a thermometer, graduated considerably above the boiling point; also two stop cocks, with joints and screws to connect with different sorts of apparatus.—Underneath was fixed a trough to contain spirits of wine, the lid perforated with two rows, of six holes each, for the cotton wicks. Being desirous of keeping the heat as equal

as possible, I fixed upon spirits of wine, because the wicks want no snuffing. There was a small pipe communicating with the trough, for regulating the supply of spirit of wine. I found it however a very difficult matter, from various causes, to keep the heat constantly equal.

My first object was to try if the water would heat through the different degrees of temperature in equal times. For this purpose I made several experiments with the boiler, sometimes half and sometimes two-thirds full of water, and all the wicks lighted. In order to save time, I generally put hot water into the boiler, and let it stand till the iron was heated equally throughout, and then began the experiment, the water usually standing at 120° or upwards. It is unnecessary to go through the detail of these experiments; but the result is, that water heats through the several degrees of the thermometer nearly in equal times; and when in a closed vessel, the same rule holds good, as well above the boiling point as below it. It may be proper to state the result of one experiment more particularly:—the water was heated from 140° to 280° in the space of 45 minutes and 31 seconds, all the 12 lights being kept burning, and the time in rising through every 10° was noted. It rose the first 10° in 3 minutes and

50 seconds, and the last 10° in 3 minutes and 52 seconds, which was the longest period.—The shortest time of passing through 10° was 2 minutes and 37 seconds. In most of the experiments, however, a little more time elapsed, as the temperature increased; but this seems to be accounted for by the greater rapidity with which the boiler cools, as its temperature increases beyond that of the surrounding atmosphere, and perhaps also by the lesser rapidity with which it heats, as the temperature approaches nearer to that of the lamps. I conceive however that it may be taken as a reasonable approximation to the truth that, by an equal and uniform application of the same quantity of heat, water will rise through the several degrees of the thermometer in equal times, and that to a temperature considerably above the boiling point, if confined in a close vessel. It appears, from the above experiments, that the temperature rises equally with the expenditure of the fuel, or nearly so, and it was before shewn, that the force of the steam increases in a much more rapid degree; but it is to be remembered, that the process is here carried on in a close vessel, and no steam escapes during the experiment, which I shall advert to more particularly by and by.

Second Class of Experiments.

I filled the refrigeratory of a still, containing exactly 300 ounce measures, with water, and noted the temperature. To the worm I fixed a flexible metal tube, which was screwed at the other end to one of the stop cocks of the boiler. I sent over steam from the boiler at different degrees of temperature, usually filling a 6 ounce measure with the distilled water, and then trying the temperature of the water in the refrigeratory again, I observed how many degrees the 6 ounces of steam had raised the 300 ounces of water. The general result of these experiments is, that steam sent over at the common boiling temperature, or within a few degrees above it, gives out as much latent heat as steam sent over at a much higher temperature, and most probably at any higher temperature whatever. The following is a more particular statement of the two last experiments I made for this purpose.

The refrigeratory was filled with 300 ounce measures of water at 55°. The thermometer in the boiler was then standing at 276°, and the stop cock being turned as little as possible, the steam was let into the worm; when 7 ounce measures of condensed water had come

over, the cock was stopped, and the temperature of the water in the refrigeratory accurately noted, and found to be 81°, being raised 26° by the 7 ounces of steam. During the experiment, the thermometer in the boiler fell from 276° to 274°, making the medium 275° for the sensible heat of the steam which passed through the worm. Had this steam contained no latent heat, it would have raised the temperature of the water in the refrigeratory only 5° and a small fraction; but it did, in fact, raise it 26°, and the result in figures gives 920° for the latent heat of the steam. If the latent heat of steam above the boiling point increased in proportion to its elasticity or force, the above experiment ought to have indicated considerably more than 2000° latent heat; but it will necessarily be remarked, that the quantity did not amount to the 940° contained in steam at the common boiling temperature, according to the authority of Mr. Watt. This difference, however, is easily accounted for by a small portion of heat, necessarily given out during the experiment from the metal tube which conducted the steam from the boiler into the worm, and another small portion from the surface of the water in the refrigeratory. In order to remove any doubt in this respect, I repeated the experiment in

the same apparatus with the water reduced to the common boiling point. Having renewed the water in the refrigeratory, and ascertained its temperature at 53°, the thermometer in the boiler standing at 212°, the steam was let into the worm till 6 ounces had come over. During the experiment, the thermometer in the boiler fluctuated between 212° and 215°. I estimated the medium, taking the whole experiment together, at 213°. When the 6 ounces of condensed water had come over, the temperature of the refrigeratory was carefully ascertained to be 74°, having been raised 21° by 6 ounces of steam at 213°. Making the calculation as before, the result gives 910° of latent heat, leaving a difference of 10° in the latent heat between this and the former experiment, which is as near as it is possible to come in experiments of this description, and with this sort of apparatus. Some of the former experiments had given the difference in favour of steam, at the common boiling temperature. I conceive, therefore, it may be safely concluded, that there is as much latent heat in a given weight of steam, raised from common boiling water, as in that of a much higher temperature; and that, when once steam is saturated with the specific quantity of heat necessary to its formation, all

further accession of heat is appreciable by the thermometer.

In attending to the progress of the preceding experiments, I observed that when the steam was let off from the boiler, at a high temperature, it passed into the worm with great rapidity, and more water was condensed in the same space of time than when the steam was sent over at the common boiling temperature. Also, when the steam at high temperature was suffered to pass freely through the stop-cock, the thermometer in the boiler began to sink with considerable rapidity.—From these circumstances, it is evident, that the steam of high temperatures is more dense than that which proceeds from common boiling water; that a greater quantity of it is compressed into a less space; and the increase of force, occasioned by the increase of temperature, no doubt proceeds chiefly, if not wholly, from this increased density of the steam, by new generation, and not at all from any additional combination of the previously existing steam with latent heat. It is a well-known law, with respect to atmospheric air, that doubling the pressure, doubles the density; but whether this law holds good with respect to steam (so that double the quantity compressed into the same space is requisite to

produce double the effect) I have not yet made any direct experiment to determine.*

In stating the latent heat of steam to be uniformly the same at all temperatures, it must be understood, with reference to the quantity of water converted from the fluid to the gaseous state, and not to the quantity of space occupied by the steam, as the actual quantity of heat increases progressively with the density. In order to obtain steam of any given density, a specific temperature corresponding with that density is also necessary; and without that temperature the steam cannot exist. If it be required to produce steam equal to two atmospheres, that can only be done by raising it from water at the temperature of 252°, or thereabouts; and if an additional weight or pressure is put upon the steam so raised, or if the temperature is lowered a few degrees, and the pressure of two atmospheres continues, in either case it will be immediately condensed into water again, although the temperature still continues several degrees above the common boiling point. The following experiment will shew this:—Take a barometer tube, hermetically sealed at one

* This seems to have been proved by Gay Lussac. An. de Chimie, Vol. 43. 1802. or Nicholson's Journal, Vol. 3. p. 267.

end, bend up three or four inches of the sealed end in the form of a syphon, introduce a drop or two of water into the tube, and upon that, a few inches of mercury; bring the mercury close up to the sealed end of the tube, so as to exclude all the air; and after letting the tube rest a little, a small portion of the water will ascend to the top of the mercury at the sealed end; let the mercury in the open leg be a few inches above the level of the closed end. If this tube is put into boiling water, and continued there ever so long, the small drop of water above the mercury will never expand into steam, because it has not only to act against the weight of the atmosphere, but also the column of mercury; and the temperature of common boiling water is not sufficient to constitute steam of an adequate force for that purpose; but if the tube is put into heated mercury, and kept there till the temperature rises to the proper point for overcoming the pressure, (for which see Mr. Dalton's table) the water will then be converted into steam, and occupy a certain portion of the tube, lifting up the column of mercury. If, before the temperature is further advanced, more mercury is poured into the tube, so as to increase the pressure of the column a few inches, the steam will be immediately condensed into water

again, and will remain so until the temperature is still further increased to the necessary point specified in the table.

JOHN SHARPE.

JANUARY, 1806.

I have subjoined the following Note, received from Mr. DALTON, which, I apprehend, will require no apology. Oct. 1810:

There are but *three* opinions, which can be entertained as at all probable on the subject of the force of steam, in contact with water, in high temperatures. 1st. Steam, over water of 252°, may be of the same *density* as that over water of 212°, and the great increase of force may arise from the increase of temperature solely. In this case, the application of steam for mechanical purposes would be much more economical, in regard to expenditure of fuel, at a high temperature. 2d. Steam, over water of 252°, may be of the same density as that over water of 212°, and the great increase of force may arise from its having combined with double the quantity of latent heat (as it has been called.) In this case, there would be no advantage in using high temperatures, except that less water would be requisite; and the preceding experiments on distillation would have abundantly manifested the truth of the supposition, by giving a much greater increase of temperature in the water condensing the steam of high temperature than in that condensing the lower. The experiments, therefore, shew the fallacy of this supposition. 3d. Steam, over water of 252°, may be of double density, compared with that over water of 212°, and the increase of

force may arise from the increase of density: in this case, it would be indifferent as to the expenditure of fuel at what temperature steam was used, because the quantity of latent heat would be as the force, or as the density: and in the distillation of water, the increase of temperature in the receiver, arising from the latent heat, would be as the weight of water distilled, without regard to the temperature of the steam.

Now, though the preceding experiments do not absolutely decide between the first and third supposition, all analogy and experience are in favour of the latter. Steam on this principle will agree in expansion with all other elastic fluids. The experiments of Gay Lussac, as well as my own, on the steam of ether, water, &c. are conformable to it; and the expansion of vapoury air and dry air by heat are found to be exactly the same, provided the vapoury air be completely cut off from the acquisition of any more vapour.

The result of one of the experiments deserves particular notice; I mean that in which it was found the temperature of the water in the boiler increased in direct proportion to the time of heating. One would certainly have expected the water to heat most quickly at first, and more slowly as the temperature advanced. I do not doubt the accuracy of the experiment; but I explain it by supposing the common thermometric scale inaccurate; the degrees of the mercurial scale are progressively too small as they ascend. See my New System of Chemistry, page 14.

ON

RESPIRATION

AND

ANIMAL HEAT.

By JOHN DALTON.

(Read March 21, 1806.)

IT is not my design, in the present Essay, to give a history of early opinions respecting the uses of Respiration, and the causes of Animal Heat. I intend to confine my observations to such authors as have written on these subjects within the last thirty years; a period in which so many discoveries respecting heat and elastic fluids have been made, as to enable modern physiologists to give a much more rational account of that important animal function, Respiration, than their predecessors could do.

Priestley and Scheele discovered that oxygen was consumed during respiration, or the quantity of oxygenous gas inhaled was greater than that exhaled. Black and Lavoisier found that a considerable portion of the air expired

consisted of carbonic acid gas; this fact, when joined to the former, led to the discovery of the true cause of animal heat, or that excess of temperature which warm-blooded animals possess, above the temperature of the surrounding atmosphere.

The striking analogy which the effects of respiration have to those of the combustion of charcoal, could not long escape the observation of Lavoisier and others. In both cases, charcoal, in a fixed or inelastic state, combines with oxygen, and produces carbonic acid gas. In combustion, a great quantity of heat is liberated, so as to raise the temperature of surrounding bodies to an intense degree; in respiration, however, little or no increase of temperature is observed, if we except the air itself, which is inspired cold and expired warm. This want of complete resemblance in the chemical effects of combustion and respiration, for a time, obstructed the progress of this branch of physiology. It was perceived that the quantity of carbonic acid produced by respiration, had it been obtained from the combustion of charcoal, would have evolved heat sufficient to preserve the temperature of the body; but the heat so evolved, if applied to the lungs of an animal, must be injurious, if not fatal. The body of a living animal is

subject to a continual expenditure of heat from the action of the surrounding atmosphere; it *must* therefore have a continual supply; an adequate supply appears to be provided by the continual combustion of the charcoal of the blood in the lungs; but how is so large a quantity of heat applied to so delicate a viscus as the lungs, without injuring it, and even without raising its temperature?

It is to Dr. Crawford we are indebted for the complete solution of this difficult question; his admirable work on animal heat and combustion will be a lasting monument of his superiority to all his cotemporaries in this walk of science.

The essential characteristics of Dr. Crawford's theory of animal heat are two; namely,

1st. That the specific heat of carbonic acid gas is *less* than that of oxygenous gas and of atmospheric air.

2d. That the specific heat of blood drawn from an artery, is greater than the specific heat of that drawn from a vein.

The former of these facts, indeed, might be inferred *à priori* from Lavoisier's experiments on the combustion of charcoal; but it was first proved experimentally by Dr. Crawford. The latter was, for aught that appears,

never so much as conjectured by any one prior to him.

According to this theory, the acquisition and distribution of animal heat is obvious: In respiration, heat is abstracted from the atmospheric air, or more properly, from the oxygenous part of it inspired, in consequence of the chemical union of elements; this heat is imparted to the blood without materially affecting its temperature, and is, during the course of circulation, given out to the rest of the body, in proportion as the blood changes from its arterial to its venous constitution.

Most, if not all, philosophers who have attended to this subject since, have adopted the two fundamental positions above laid down, which have never, I believe, been controverted by any one; and, whilst they continue to be admitted, it would be in vain to frame any other theory in order to account for animal heat.

Notwithstanding this general agreement as to the source of animal heat, there are still various opinions respecting the mode of those chemical changes that take place in the air and in the blood in consequence of respiration. Before we can animadvert upon these, it will be necessary to premise, that the air of the atmosphere *inspired* consists of

azotic gas, oxygenous gas, aqueous vapour, and a very small quantity, almost inappreciable, of carbonic acid gas; that the air *expired* consists of azotic gas nearly the same as before, oxygenous gas diminished in quantity; and carbonic acid and aqueous vapour, both considerably increased in quantity; the temperature of the expired air, as is well known, is in most instances much superior to that of the inspired air.

Lavoisier and Crawford, followed by many respectable writers, seem to maintain, that the basis of carburetted hydrogen gas transpires through the thin membranes of the lungs, from the blood, where, meeting with the oxygenous gas of the atmosphere, a chemical union of the carbone and hydrogen with the oxygen takes place, forming carbonic acid and aqueous vapour; at the same moment, part of the heat of the oxygenous gas is given out, which, according to Crawford, enters the blood of increased capacity for heat, and consequently does not materially increase its temperature. This heat is again given out during the circulation, as has been observed, in order to supply the waste from the body.

In order to establish this explanation, it is necessary to shew, that the oxygen disappearing is just sufficient to form the carbonic acid

and the aqueous vapour. Upon a careful examination of the facts, however, the results do not form a true equation; the quantity of aqueous vapour exhaled is undoubtedly greater than can be accounted for as above; the excess of vapour is supplied, we may suppose, by the natural exudation of moisture through the thin membranes of the lungs.

In the Annales de Chimie for 1791, about three years after the 2d Edit. of Dr. Crawford's book, we find a memoir by Hassenfratz on the subject of animal heat.—In the course of the memoir, M. de la Grange is introduced as objecting to Crawford's theory, because it supposes all the heat to be given out in the lungs, which, he thinks, would be in danger of consuming them; he finds it expedient, therefore, to invent *another* theory, as he conceives, in which the heat may be *gradually* given out, during the course of the circulation, to all the parts of the body.—It is scarcely possible for any one, who understands the doctrine of Crawford, to read the observations of La Grange, and his commentator, Hassenfratz, without smiling at their palpable ignorance of the doctrine under their review. The distinguishing feature of Crawford's theory is, *that of the greater capacity of arterial blood for heat, than of venous*

blood, by which the large quantity of heat can be received into the lungs without at all raising their temperature. This object is precisely what La Grange and Hassenfratz have had in view by their new theory; notwithstanding their pretended objections, they, in reality, adopt the very same principles which Crawford had the merit to discover.

The change they propose to make is this; the oxygen inspired, instead of entering immediately into combination with the carbone and hydrogen, as Crawford supposes, enters first of all into the blood, without depositing much of its heat; during the circulation, this oxygen gradually combines with the carbone and hydrogen, forming carbonic acid and water, and giving out heat in consequence, till the blood, on its return again to the lungs, throws out the carbonic acid and water, and receives a fresh supply of oxygen. Every one must see, that these positions are necessarily dependent on the two essential characters of Crawford's theory; namely, that of carbonic acid having a less capacity for heat than oxygenous gas, and that of arterial blood having a greater capacity for heat than venous blood.

Instead, therefore, of pulling down the ingenious edifice erected by Crawford, and building another in its place, as they imagine;

the whole change effected consists in removing the cornice, and substituting another in its place. We must now enquire in which state the edifice presents the most symmetrical appearance.

According to La Grange and Hassenfratz, oxygen enters the blood in the lungs. How does it enter? By *mechanical* or *chemical* means? Not by mechanical; for then azote would enter four times more copiously, owing to its greater density. It must enter by chemical means.—How does the blood attract oxygen through the membrane of sensible thickness which separates them? Granting the fact, how does the elastic fluid enter into combination with a liquid, without depositing its heat in the lungs, a circumstance so much to be guarded against on this hypothesis?—If the heat be given out to the blood in the lungs, there will be none left to be extricated during the circulation in order to form carbonic acid. Passing by all those difficulties, how is the carbonic acid to escape through the membranes of the lungs into the air cells? Not by *chemical* means, for there is no agent to attract it; mechanical means must be used; simple pores will not effect the business, because air might enter as well as escape; there must then be air pores with valves opening

outwards so as to permit the escape, but bar the entrance of any gas. These pores, I am afraid, would be so constantly filled with liquid, that it would obstruct, if not altogether destroy, their proper function.

The whole scheme is evidently attended with insuperable difficulties. But it will be urged, that the blood has a known affinity for oxygen; witness the florid colour which it always assumes in oxygenous gas. True; but does this prove that oxygen has combined with the blood, and entered into that liquid, or does it prove that some particles of the blood have combined with oxygen, and made their escape from the surface of the liquid, which assumes a vermillion hue after their departure? I apprehend this question has not yet been determined: Mr. Davy informs us (Researches, page 381,) that venous blood, agitated with atmospheric air and oxygenous gas, assumed the vermillion colour at its surface; "*but no perceptible absorption had taken place.*"—Here then we have a change of colour without sensible absorption when the blood is in contact with the gas; is it probable then that an absorption will take place when the blood is separated from the gas by a membrane of considerable thickness?

It is somewhat remarkable, that this supposed amendment of Crawford's theory should have been so generally adopted. The authors of it evidently did not understand the principles they were attempting to refute; their objections to them may be applied with equal force against their own principles; they obtain the very same end by means much less probable: yet the physiological writers of this country have almost universally embraced their innovation upon the original system. I cannot ascribe this to any other cause than that unwarrantable neglect of cultivating the doctrine which instructs us respecting the capacities of various bodies for heat. Having now given my own views of the present state of the *theory* of Respiration and Animal Heat, I shall proceed to make a few observations upon the facts and experience relative to this subject, since the time of Crawford.

Davy, Henderson and Pfaff have almost established the fact, that a small portion of azotic gas disappears by respiration; this escaped the notice of Lavoisier and Crawford, who seemed to have concluded, that oxygenous gas was the only part of the atmosphere changed by breathing. Whatever other use may be attached to the fixation of azote in

the system, one is evident, namely, its contributing to the support of temperature in the same way as oxygen does.

Since the late improvements in Eudiometry, attempts have been made to determine with greater precision, the changes effected by respiration in the elastic fluids. It is obviously of importance to learn the precise quantities of oxygenous gas inspired and expired, together with the quantities of carbonic acid, and aqueous vapour expelled from the lungs. With respect to oxygen and carbonic acid, my own experience concurs with that of the generality who have carefully investigated the subject; more in bulk of oxygenous gas is consumed than that of carbonic acid generated; the former appears to be about 5 per cent. upon all the gas inspired; the latter about 4 per cent. upon all the gas expired. It is very desirable, but at the same time very difficult, to determine the ratio more exactly. It ought to be observed too, that the quantities above specified are the medium for each one natural expiration; if the gas at the first moment of expiration be caught, it will be found to contain about 3 per cent. acid, and to have lost 4 of oxygen; but if the last portion be examined, it usually contains 5 of acid, and wants 6 or more of oxygen; by taking the last gas of a

forced expiration, I find it to contain 6 per cent. of acid, and to have lost nearly 8 per cent of oxygen.

By frequent trials, I find the quantity of air taken in at each natural inspiration by me, is about a pint, wine measure, or nearly 30 cubic inches. This quantity is considerably less than some authors state it, and more than others. It is probable, that different subjects exhibit a difference in this respect; but it can scarcely be so great as is represented. I find, too, that in a state of quiescence, I take 20 inspirations in a minute. This gives 500 cubic feet of atmospheric air inspired in a day, =46,5lbs. troy, of which 105 is oxygenous gas, and 25 of this enters into new combinations. This will be found to weigh 15120 grains=2,6lbs. troy.—By a full forced inspiration, my lungs can contain about 7 pints or 200 cubic inches of air, which can be expelled again by a forced expiration; the quantity still remaining in the lungs, after such expiration, is not easily to be determined; it cannot however be much, and it is of little consequence to know it exactly. It appears then, that after an ordinary expiration, my lungs still contain 3 pints of air; and that after an ordinary inspiration there is still room left for 3 pints more.

The quantity of carbonic acid gas expired in a day may be calculated thus: the whole quantity of gases expired in a day being as stated above =46,5lbs. troy, and 4 per cent. or $\frac{1}{25}$ of this in bulk, being carbonic acid, we have $\frac{46,5}{25}$=1,86 lbs.; but carbonic acid being $1\frac{1}{2}$ times the weight of an equal bulk of common air, we have 2,8 lbs. troy for the weight of carbonic acid gas expired in a day.

There is a considerable diversity in different authors, and even in the same author at different times, respecting the quantity of carbonic acid, obtained by respiration. Lavoisier, in his first memoir in 1789, and Davy, nearly coincide with the results I have given above from my own experience. Afterwards, it seems, that Lavoisier made the quantity much less, not one half of the above. I cannot conceive what could induce him to rate it so low. On the other hand, Dr. Menzies estimates the quantity nearly 4lbs. troy; which, I think, must be above the medium for men in general.

The quantity of aqueous vapour exhaled from the lungs in a day has been variously estimated; and a greater uncertainty respecting it subsists at this moment, than respecting any other product of respiration. Dr. Hales, by experiment, found that 20 oz. per day were

expired; Dr. Menzies found 6 oz.; Mr. Abernethy, 9 oz. Lavoisier, partly by experiment, and partly by theory, in one of his memoirs, estimates the water exhaled from the lungs daily at 28½ oz.; but in some instances, he estimates more, in others, less.

This diversity of results amongst the earlier physiologists was not to be wondered at; but it is somewhat surprizing, that after the recent discoveries on the nature of steam or aqueous vapour, any material uncertainty should still remain respecting the quantity of water exhaled from the lungs in a given time. Nothing is more obvious and easy than to calculate, *à priori*, the precise quantity of aqueous vapour in a given quantity of air expelled from the lungs. At the temperature of 98°, the utmost force of aqueous vapour is nearly equal to 1¼ inches of mercury, as appears from Tables of various authors.* The force of aqueous vapour existing in the atmosphere is various; but the medium quantity in this climate may be estimated at 30 of an inch of mercury, due to the temperature of 44°. (See Memoirs, vol. 1. second series, page 263.)

Now it is certain that the air in the small

* See Memoirs, vol. 5, page 560. Bettancourt's Experiments in Encycl. Brit. or Hutton's Math. Dic. &c.

ramifications of the air-vessels of the lungs, surrounded by moist membranes, must, in a moment, be nearly saturated with vapour; we shall have, therefore, an increase of the force of vapour from that inspired, .30 to that expired of 1,74 inches of mercury, being an increase of 1,44 inch. But by reason of the less specific gravity of vapour than air, in proportion as 7 to 10, vapour of the above force will only be equal in weight to air of 1 inch of force. Hence the weight of aqueous vapour exhaled at any time must be nearly equal to $\frac{1}{30}$ of the weight of the whole mass of elastic fluids expired. We have then $\frac{46.5}{30}=1,55$ lbs. troy for the weight of aqueous vapour expired in a day, on the supposition that $46\frac{1}{2}$ lbs. of air, &c. are expired, and that the air so expired is saturated with vapour, or contains as much as any gas can do in the temperature. The real quantity expired can not exceed that stated above; nor is it probable that it can fall much short of it.

It is worthy of remark, that Dr. Hales, who was one of the earliest to investigate the quantity of water exhaled, should have approximated nearest to the truth; and that he should rather have exceeded the truth in consequence of his alkali extracting, not only the additional vapour acquired in the lungs,

but a portion of what was previously in the air.

We may now deduce one conclusion, which indeed Lavoisier was fully aware of, that the oxygen which disappears during respiration, is not adequate to the formation of the carbonic acid and the water exhaled. It is only $\frac{3}{4}$ of the requisite quantity. He conceives only a part of the water is formed in the lungs by the union of oxygen with hydrogen from the blood, while the rest transpires ready formed, through the membranes of the blood-vessels, and is vapourized by the heat.

This indeed is the most difficult part of the subject. I am inclined to think, that no water is formed in the lungs by the union of oxygen with hydrogen; but that the whole quantity exhaled is an exudation from the blood, through the membranes of the lungs, which are thereby constantly kept moist.—It is inconsistent with the simplicity of the laws of nature to employ two causes when one is adequate to the effect. There is another way by which the difficulty may seem to be obviated; that is, by supposing that all the water exhaled is formed in the lungs by direct combination of its elements, but that the carbonic acid is formed from carbonic oxide, which has previously one half of the oxygen necessary for

the acid. On this supposition, the oxygen would be sufficient for both; and we must consider a triple compound of carbone, hydrogen, and oxygen, to transude through the lungs, which is to be converted into carbonic acid and water. This explanation would not differ essentially from that given by Lavoisier and Crawford; which supposes that nothing enters the blood in respiration; but that the combustible matter unites with oxygen on the surface of the lungs. The position seems to require that whenever carbonic acid is generated in the lungs, a certain portion of water must be generated at the same time; I doubt whether this is consistent with facts. It is well known a person may, for some time, breathe with impunity, air containing more aqueous vapour than that ordinarily expired; yet carbonic acid continues to be formed nearly as usual. I have been for 10 minutes in a stove where the temperature was 140°, and where the vapour inspired was more abundant than that expired; yet the air expired at the conclusion of that time contained 3 per cent. of carbonic acid, and had lost 4 per cent. of oxygen, nearly as usual; and no superabundance of vapour was perceived on the lungs. Having made some comparative trials upon air that has been breathed, and air in which charcoal

has burned out, I am almost convinced that the changes effected by these processes are the same; and consequently am inclined to believe that all the oxygen, which disappears in the lungs, goes to form the carbonic acid produced, whilst the heat liberated enters the blood for the purpose of preserving the temperature of the body.

But it will be said, there is more oxygen spent than is requisite for the carbonic acid; what then becomes of the surplus? In answer to this I would observe, that the fact stated in the objection must first be ascertained.—According to Lavoisier, whose results have been since corroborated by those of Clement and Desorme, 28 parts of charcoal, by weight, unite with 72 of oxygen, to form carbonic acid; in this case, a given volume of carbonic acid contains almost exactly the like volume of oxygenous gas; whence the objection would have validity. But Crawford, (page 343) finds 20 of charcoal unite to 80 of oxygen to form carbonic acid; in this case, 4 measures of carbonic acid will be found to contain 4,68 measures of oxygenous gas, or 6 contains 7 nearly; and the proportion will come very near to that observed as the effect of respiration; the difference is so small as may easily be attributed to inaccuracies, even in the present improved state of Eudiometry.

APPENDIX.

(Read November 16, 1810.)

As considerable time has elapsed since this paper was read (in 1806), and several important memoirs have been published on subjects nearly related to the present, the committee has given me leave to make such additional observations as may be judged expedient.

At the time of writing the preceding memoir, I had not seen a judicious collection of facts and observations on respiration, by Dr. Bostock, published in 1804. From a careful comparison of the results of physiologists, at that period, he draws, amongst others, the following conclusions:

1. Air loses near 4 per cent. in bulk of oxygen by being once respired; a man consumes about 2 lbs. 8 oz. in 24 hours, or 26 cubic feet.

2. The carbonic acid generated by respiration, is 82 for 100 oxygen in volume; and consequently, from the known constitution of carbonic acid, it cannot contain all the oxygen which disappears. The weight

of carbonic acid formed in 24 hours is about 3 lbs. which are equal to 22 cubic feet.

3. A quantity of aqueous vapour, the amount of which is still undetermined, is emitted from the lungs.

In July, 1806, after the preceding paper had been read, I instituted a series of experiments on respiration, and on the combustion of charcoal, oil, &c. by the results of which I became convinced, that the changes made in common air, by the combustion of charcoal, and by respiration, are the same. I find the following note made on the 4th of July:—"The result of all these experiments is, that breathed gas and gas in which charcoal has been burnt, are the same in regard to acid and oxygen, and that the acid is either equal to, or rather less than, the oxygen in its composition." Since that time, I have made no more experiments relative to the subject. The substance of this note was soon after communicated to Dr. Thomson, who published it in the 3d Edition of his Chemistry, 1807, and corroborated it by the results of some subsequent experiments of his own. Though it had appeared from the experiments of Crawford, Menzies, and Davy, that the carbonic acid produced in respiration was equal, or nearly equal, to the oxygen consumed (in

bulk); yet it was most commonly supposed, that the experiments of Lavoisier were more to be depended upon. At least, the above conclusions of Dr. Bostock, and the account which Mr. Murray has given in his Chemistry, 1807, seem to warrant the observation. The last gentleman adopts the proportion of 84 acid for 100 oxygen in respiration.

In the Phil. Transac. for 1807, Messrs. Allen and Pepys have given a very excellent paper on the quantity of carbone in carbonic acid, and on the nature of the diamond. (See also Nicholson's Journal, vol. 19, 1808.) These authors, to all appearance, indisputably confirm the results of Lavoisier, in regard to the constitution of carbonic acid; namely, that it is a compound of 28 parts of carbone by weight, and 72 of oxygen, or very nearly so; and that carbonic acid contains just its own bulk of oxygen.

The Phil. Transac. for 1808 (or Nicholson's Journal, vol. 22.) contain a very laborious, and apparently, accurate series of experiments on respiration, by Messrs. Allen and Pepys. After a great number of experiments, made under advantageous circumstances, with the experience of previous enquiries before them, and with improved methods of analysis,

they deduce a number of important results. The first and the principal one is, *that the quantity of carbonic acid gas emitted is exactly equal, bulk for bulk, to the oxygen consumed.* This is the same conclusion as I had obtained; it amounts almost to a demonstration, that the oxygen which disappears is spent wholly in the formation of carbonic acid; though it is possible to conceive that *one half* of the oxygen unites to carbonic oxide, from the lungs, and the other half to hydrogen, from the same source, thereby forming both carbonic acid and water, agreeable to the notion of Lavoisier and Crawford. This last position, however, appears to me highly improbable. The authors do not produce any decisive experiments, nor give an opinion, respecting the question, whether the oxygen combines immediately with the carbone presented to it, as supposed by Crawford, or, on the other hand, the oxygen combines with the blood, and in the process of circulation, carbonic acid is formed, which is given out in the lungs, as La Grange and Hassenfratz would have it. Messrs. Allen and Pepys estimate the carbonic acid emitted in a day by a middle-sized man, to be about $3\frac{1}{4}$ lbs. troy. They establish a fact, that before was doubtful, viz. that in ordinary respiration no material absorption or

evolution of azote takes place; and another, that no carbonic oxide is ever found in respired gas.

In a series of experiments on respiration, published in the Phil. Transac. 1809, by the same gentlemen, (see also Nicholson's Journal, vol. 25.) several curious and interesting results are obtained. Among them are some to the following purport: 1. That when pure oxygen is respired, a portion of it is missing at the end of the experiment, and its place supplied by a corresponding quantity of azote. 2. That a mixture of 78 hydrogen and 22 oxygen may be inspired for an hour or more; it tends to produce sleep; at the end, a deficiency of hydrogen and corresponding increase of azote are observed, more than can be ascribed to the uncertain capacity of the lungs. 3. That the lungs of a middle-sized man contain more than 100 cubic inches of air, after death.

From these results, it should seem, that any air which can be respired would lose a portion in the process, and acquire an equal portion of azote; this may, perhaps, be occasioned by the blood parting with the common air, which it contains mechanically; that is, in the same way that water and other liquids contain it, and receiving a portion of the

other gas, agreeably to the principle of pressure established by Dr. Henry. But the quantity of gases thus interchanged was too large in some of the instances to admit of this explanation, unless there was some inaccuracy in the experiment.

The above accurate experimentalists have not yet published any enquiry concerning the quantity of steam or aqueous vapour produced by respiration. If they should think the theoretical determination in the preceding pages insufficient, namely, $1\frac{1}{2}$lb. in a day per man, it is to be hoped they will endeavour to ascertain the facts experimentally, being well qualified for the purpose, and having an apparatus superior to most or all of their predecessors in this department of science.

From these additional remarks, it will be understood, that the leading principles of Crawford's theory of animal heat remain yet in nearly the same state in which he left them. Several of his subordinate facts have been either corrected or ascertained with greater precision; for instance, the proportion of carbone and oxygen in carbonic acid, which he deduces as 1 : 4, has been found as 1 : 2,6; the change made in respiring common air has been found to resemble that made by burning charcoal rather than wax; the quantity of the

aqueons vapour expired has been shewn and its source explained in a way contrary to his view: But that arterial blood has a greater capacity for heat than venous blood, that oxygen gas has a greater capacity for heat than carbonic acid gas, the two great pillars on which his theory is supported, remain untouched; indeed his results in regard to these points are so plausible, and his whole theory so beautiful, that one would feel a regret in having to question the accuracy of his principles.

On the gradual Deterioration of the Atmosphere, by Respiration and Combustion.

IT is now upwards of 20 years since Dr. Priestley published an Essay, "On the Restoration of air infected with Animal Respiration and Putrefaction, by Vegetation."* After remarking that candles will burn only a certain time, and animals live only a certain time, in a given volume of atmospheric air, the air being rendered noxious by those processes, he adds, "I do not know that any

* Experiments and Observations abridged, vol 3. page 255.

" methods have been discovered of rendering " it fit for breathing again. It is evident, " however, that there must be some provision " in nature for this purpose, as well as for " that of rendering the air fit for sustaining " flame; for without it the whole mass of the " atmosphere would, in time, become unfit " for the purpose of animal life; and yet there " is no reason to think that it is at present at all " less fit for respiration than it has ever been." In the sequel, he concludes, from certain experiments on vegetation, that it is one of the processes employed by nature for the great purpose of restoring the atmosphere to a fit state for the support of respiration and combustion. How far this conclusion is correct, namely, that the growing of vegetables abstracts the carbonic acid from air, I have had no opportunity to observe. But the necessity of this, or some other process, for the purpose, has, I believe, been generally adopted by the later writers on this subject. No one, that I know of, has undertaken to calculate the quantity of carbonic acid, which is probably thrown into the atmosphere in any given time, in order to compare it with the whole quantity of the atmosphere. Now, if we state the diameter of the earth to be 8000 miles, and the circumference 25000, in round

numbers, the area of the earth will be 200 millions of square miles: calculating the weight of the atmosphere at the rate of 15 lbs. upon a square inch; for such a number of miles we obtain 12 trillions of lbs. avoirdupoise;—calculating also the quantity of carbonic acid which 1000 millions of men, (the supposed population of the earth) would expire in the space of 6000 years, at 3 lbs. per day, we shall find it to be 6 thousand billions of lbs. or just $\frac{1}{1000}$ part of the whole atmosphere: now, supposing this doubled, to allow for the quantity of acid which may be supposed to be generated by combustion, we shall then have $\frac{1}{1000}$ part of the atmosphere to be carbonic acid, which agrees with experiments as to the quantity now actually found in it. There is not therefore any necessity to believe from the phenomena, that means are used by nature for the restoration of the purity of the atmosphere.*

* Since this paper was sent to press, I have had an opportunity of making a few comparative experiments, the results of which deserve notice. Hearing of a young person living upon simple diet, and taking no fermented liquors, who feels cold very sensibly, so as to require warmer clothing, and who is obliged to avail himself of artificial heat, more than others; I was desirous to learn

how far the above circumstances might be connected with the function of respiration. We found that each of us breathed at an average 20 times in a minute, but that the quantity of air which he expired each time, was only two thirds of that which I expired. The capacities of our lungs appeared to be in the same ratio of 2 to 3; for, the whole quantities of air which each of us could expel from our lungs, both after a natural and forced inspiration, were as nearly as we could determine in that ratio. The quality of the air expired by us was found to be the same, both in the natural and forced expirations; in the former case the air contained 4½ per cent. of carbonic acid, and 16 of oxygen, and in the latter, 7 carbonic acid, and 13 or 14 oxygen. The size of our persons is nearly the same. The experiments were made in August, in a temperature of 60°. Now if the quantity of heat generated, or more properly speaking, acquired by the animal system, be in direct proportion to the carbonic acid expired from the lungs, as all experience would seem to warrant from its evolution, the above results are consistent therewith, and the facts admit of a satisfactory explanation.

I have just seen a paper in the Philosophical Transactions for the present year (1811), by Mr. Brodie, containing some facts affecting the theory of animal heat. It is entitled "Physiological Researches respecting the Influence of the Brain on the Action of the Heart, and on the Generation of Animal Heat." There is an important addition to it in a subsequent paper of the same author. From his experiments the author deduces the following conclusions:

"1. The influence of the brain is not directly necessary to the action of the heart.

"2. When the brain is injured or removed, the action of the heart ceases only because respiration is under its influence, and if under these circumstances respiration is artificially produced, the circulation will still continue.

"3. When the influence of the brain is cut off, the secretion of urine appears to cease, and no heat is generated; notwithstanding the functions of respiration and the circulation of the blood continue to be performed, and the usual changes in the appearance of the blood are produced in the lungs.

"4. When the air respired is colder than the natural temperature of the animal, the effect of respiration is not to generate, but to diminish animal heat."

Mr. Brodie seems to doubt from the above conclusions, and from sundry observations in the paper, whether respiration is the source of animal heat. But it seems premature to draw conclusions respecting the source or *acquisition* of animal heat from experiments relating to its *evolution*; the two functions by which these processes are carried on may be variously affected in such extraordinary circumstances as those above alluded to, and there may not be that mutual and correspondent action which takes place when the animal is in full possession of all its vital energies. It should appear from the last experiment (though the results are not ascertained with the requisite accuracy) that the acquisition of heat goes on in some degree; for carbonic acid is generated; but the secretion of heat, like that of urine, is totally suspended. It is somewhat remarkable, that in all the experiments previous to this, in which a comparison of the venous and arterial blood was made, (the 1st, 2d, 3d, 5th and 6th) the blood in the arteries was seen of a florid red, and that in the veins of a dark colour; but in the 9th experiment, when oxygen gas was inspired, and the production of carbonic acid observed, "the blood in the arteries was very little more florid

of carbonic acid formed in 24 hours is about 3 lbs. which are equal to 22 cubic feet.

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* Experiments and Observations abridged, vol 3. page 255.

and hunting forests, a large quantity of the surface of the earth must be appropriated to their enjoyment, without the intermediate step of population. If on the other hand they hold such pleasures in contempt, and only study the most effectual measures for increasing the number of their servants and dependents, or if they consume such articles of commerce as require much human labour for their manufacture, and little or no land for the production of the raw materials, their wants will operate as a certain cause of the most productive agriculture.* This process has no direct dependence on the state of improvement of other arts. When the state of manufactures is low, the wealth of a country is proportionally insignificant; but if the manufactured commodities are in demand among landed proprietors, or their dependents, every cause that promotes the cultivation of the ground, and the population of the country, exists in full activity. When manufactures are highly improved, and internal commerce regular and brisk, society becomes wealthier, but the rate of population

* If we could suppose the views of landed proprietors to be perfectly harmonious, formed on principles of independence, and directed by sagacity, they would effectually regulate both the degree and the kind of population that would exist in every country.

is not different. The proprietor of land still possesses the original command of human labour. The capital and credit of the merchant reduce the employment of this labour to a system. By thus rendering it more productive, he adds to the conveniences of the landed proprietor, and he himself is also furnished with the luxuries of life. An increase takes place in the proportion of persons who live in affluence; but none in the sum total of population.

Mr. Spence errs in considering commerce as a necessary spur to agriculture. In a country destitute of commerce, the passion of men of influence for increasing the number of their vassals, would produce the same effect. This principle formerly supported tillage in some districts of our country from which it is now excluded. Formerly a chieftain was as well satisfied when his brave hordes supported themselves amidst inaccessible mountains as when they subsisted on the produce of the open plain. Now, land will yield no profit in grain, unless manufactures are brought to its neighbourhood, or means found to convey its produce to a distance. On this account, under such circumstances, grain is not raised. We thus find, that the commercial spirit has had a

discouraging influence on some branches of agriculture; and we formerly showed, that, where men are regulated by motives of luxury, those who aim at the enlargement of their fortune, by the improvement of land, will have equally powerful motives in the lowest, as in the highest state of commerce. If it is said that commerce improves land, by enabling merchants to accumulate profits which are often expended in agricultural improvements, it should be recollected that this effect of commerce is extremely limited; and that the same effect would be produced by habits of virtuous parsimony, or by a regular system of credit, established on landed security.

It sometimes happens that an improvement in the useful arts threatens to injure population. When new machinery is invented which supersedes the greater part of the labour employed in a particular branch of manufacture, many elderly persons, who are unable to change their mode of employment, are reduced to indigence; and even the active labourer is unemployed for a time. The latter however, is certain of finding employment in a short time in some other department. The reason of this is, that the article of manufacture prepared by means of the improved machinery, is reduced in price; and the persons

who consumed it, whether landed proprietors or their servants, or manufacturers, have it now in their power to purchase some other article with the overplus of their income. The manufacturer and the merchant, perpetually watching the state of the market, observe what particular article comes into demand, and direct their labourers accordingly. If the article thus extended in its sale requires no additional extent of land for the production of the raw material, the change produces no ultimate effect on population: if otherwise, population is diminished.

These considerations enable us to form a ready estimate of the influence of foreign commerce on population. The theorem on this subject may be reduced within very narrow bounds. Whenever an article manufactured for the foreign market requires for the production of the raw material a portion of our own soil, which is capable of producing food, the tendency of foreign commerce is to diminish our population, except in so far as it is compensated by an equivalent importation of the necessaries of life. Where an article is manufactured for the foreign market, from foreign raw materials, or where the materials are procured from subterranean mines, from the sea, from land incapable of producing

human food, or from a substance which otherwise would exist as mere refuse, foreign commerce cannot possibly injure population; and if it procures an importation of food, in return for the export now mentioned, its effect must be to extend it. This effect is most likely to take place where a nation that enjoys a free trade excels its neighbours in the ingenuity and industry of its manufacturers, because a given quantity of goods is produced by a share of exertion comparatively moderate, and procures a liberal return in the produce of other countries. If a trade, under such circumstances, is sufficiently long continued, a part of the return will be given in the form of food, or other articles of necessity, for the support of an additional population.

A mere increase of population, however, is not one of the most liberal objects of political œconomy; and, when it is procured at the expence of a large portion of misery, it is to be sincerely regretted. To add to the happiness of a people, is far more desirable than to swell their numbers. If the increase of happiness could be proved to be the invariable consequence of the extension of foreign commerce, that would be the best possible reason for setting a high value on it. There is no doubt that its tendency is in general favourable

to immediate gratification, as it affords a choice of pleasures. It ought therefore to be highly conducive to happiness, and cannot, in fact, be the cause of misery among a people, except in consequence of some perversion of their taste. When any perversion of this sort is general, any event that would deprive a country of their favourite gratifications, would certainly prove a blessing, however much it might diminish the wealth or the sum total of exchangeable value. Mr. Spence, however, is unfortunate in selecting the importation of wine and foreign spirits into Great Britain, as an instance of this sort. If the importation of these articles were prohibited, the immediate consequence would be, that those who now send their native produce abroad, to pay for these luxuries, would convert more of their own grain into intoxicating liquors. The same scope would thus be afforded for hurtful excess. When we enquire into principles of conduct, we should presume, that men, when well informed, will make a prudent choice. In an enquiring age, we should suppose, that they will become practically enlightened by the influence of moral research. On this principle, we should pronounce foreign commerce to be favourable to the happiness of a country. At the same time, we must not

forget that national happiness is far more powerfully affected by circumstances totally independent of it. It depends so much on the degree in which a nation enjoys freedom of principle, political equity, and social order, on the general diffusion of the comforts of life, the prevalence of virtuous habits, moderated desires, kind affections, and cultivated manners, that, in comparison, the effects of foreign commerce almost entirely disappear.

Foreign commerce, in its present state, is attended with some causes of unhappiness, which it is to be hoped are not inseparable from it. The unhealthiness of various processes for preparing goods for the foreign market, is a very important subject, and has been frequently adverted to. The moral effect of trade, in general, on the human character, ought also to be seriously considered. It is the error of some to extol it indiscriminately as the cause of industry, and to hail its profits as the rewards of merit. It ought to be remembered, that its profits are often equally fortuitous with the sums acquired by gaming, or by lottery. They are not indeed so pernicious in their tendency, because they have a connection, though somewhat loose, with industry. But the chances of high success on the one hand, and the risks of failure on the

other, tend to give a vague character to the hopes of the trader, and generate a spirit of adventure which often leads to disappointment. An industry of a steadier and happier kind would be produced, if a constant attention to business were attended with a slower augmentation of fortune, exempt from all risk of disappointment. The mind, in that case, no longer injured by excessive passions, would be invited to relish life, by the uniform encouragement which it would afford.—Perhaps the state of commerce may, at a future period, be in this point of view improved. The frivolous caprice of fashion among the rich, may give place to a taste for more steady enjoyments; and thus a more uniform demand for the products of the useful arts may be created. An improvement may also take place in commercial sagacity, which will enable the merchant more easily to foresee the fluctuations of the market, and prevent the derangement occasioned by unexpected changes.

Let us now consider the influence of foreign commerce on national power. This part of the subject is at present peculiarly interesting, as the posture of the affairs of Europe threatens to bring the principle to the test of experience,

in the case of Great Britain. We are threatened with the total loss of our foreign commerce: and, if our power depends on it, the accomplishment of the threatening will involve the destruction, not only of all that from political habits we reckon dear, as an independent nation, but of the more substantial blessings of domestic peace and security. It is therefore highly interesting for us to know in what degree our power can be supported by our own native resources.

The power of a nation depends chiefly on the defensible state of its territory, the extent of its population, the facility with which that population can be called into the public service, and its degree of knowledge and dexterity in the art of war. Some of these circumstances have evidently no dependence on foreign trade: in others, its influence is, in the present state of our knowledge, somewhat problematical. The argument has very properly been made principally to rest on the influence which it possesses in enabling us, through the medium of an extended taxation, to call out our population into the public service. Some have asserted that foreign commerce is a separate source of revenue; others that it is merely a more circuitous method of taxing the produce of land and

domestic labour. As illustrations of these two different modes of thinking, we quote the opinions of Mr. Spence, and those of the critic who replies to him in the Edinburgh Review.

Mr. Spence pronounces it absurd to consider any branch of our commerce as deriving importance from the duties which are levied on it. All such duties, according to him, are finally paid by the consumers of the articles on which they are laid, and these consumers are equally able to pay the sums they advance, whether they consume such articles or not. If the present consumers of tea and wine, for example, were to drink nothing but water, they would possess not only the same, but a considerably greater, power of contributing taxes for the exigencies of the state.

To this the reviewer replies, that the appetite which men have for the luxuries on which the taxes are laid, is the sole cause of the production both of the luxuries themselves, and of the taxes which they bear: and, therefore, if the incitement is withheld, industry and production must infallibly languish. That it is not enough for the Chancellor of the Exchequer to recommend to the people to leave off tea and wine, that they may be better able to

pay taxes, and make voluntary contributions, unless he has power to persuade them to take as much pleasure in earning money for the service of the state, as in consuming these luxuries.

There are two modes of attempting to produce a political effect through the medium of public discussion. Some direct their doctrines entirely to the candour of statesmen, and consider the existing habits of the community at large as facts which must be taken as they are found, without depending on the possibility of moulding them to particular purposes. Others address the mass of society as consisting of persons, who, when once informed of their interests, may be roused to patriotic feelings sufficient to make them cheerfully submit to great privations. It is not necessary to determine whether statesmen or the rest of the inhabitants of this country, exhibit the greatest patriotism and self-denial, or which of the two classes is most slavishly tied down by immediate appetite, and by the homage demanded by the caprice of fashion. But the prevalence of public virtue in either class of persons, would certainly promote it in the other. Where a nation is universally patriotic, there exists the greatest spur to patriotism among statesmen; and, on the other

hand, if the leading men in government show a disinterested patriotism, the people, conceiving their interests lodged in safe hands, will feel the best encouragement to cherish a spirit truly patriotic. An improvement in the sentiments of each is most likely to advance, by their going hand in hand. Political writers should address their doctrines to both alike. It is injudicious to impress the one with an opinion of the untractableness of the other. If it is difficult to excite among the inhabitants of this country an interest in its fate fully adequate to make them submit to privations as well as to hazards, this certainly proceeds from some unfortunate want of mutual confidence and of cordial co-operation, rather than from any invincible attachment to immediate ease and pleasure, and no such principle ought to be adopted as a fundamental position by writers who are endeavouring to unite their countrymen by enlightening their minds. In this point, the reviewer appears to be deficient. He is also chargeable with some inaccuracy in estimating the operation of the love of luxury, even supposing it to continue as predominant as it now is.

That sentiments of disinterested patriotism must be fully exerted before the government can avail itself of their existence, is so far

just; but in the illustration which the reviewer subjoins, he shows himself in some measure aware of the reply to which the application of that argument was open. He adds, "Nor do we think that Mr. Spence will succeed in convincing the people of England to go without wine, and to hoard Birmingham manufactures." This also may be true: but it only shows that the taste of consumers may not take that turn which a particular author might recommend. It cannot probably be directed, and its changes may not be easily foreseen. But there is one principle in human nature to which it is of the utmost importance for us to attend; that as long as a spirit for active labour exists in the country, and as long as that spirit is encouraged by a love of luxury among the rich, those of the latter, who are deprived of the opportunity of purchasing one luxury, will find some means of spending their superfluous income; and these means will call into a different field of exertion, the labour of those persons who were employed in importing luxuries from abroad, or in preparing manufactures to pay for them in the foreign market. The most serious disadvantage that arises, consists in the temporary embarrassment produced by the sudden change given to the great machine of commerce, and the uncertainty

which, in the first instance prevails, respecting the direction which general consumption may take. It is the business of the financier to discover, as soon as possible, to what particular quarter the consumption is directed, and from whence the revenue may be advantageously raised. It is vain to object, that the destruction of foreign commerce leaves no superfluous income, and only deprives of their income those persons who were engaged in it. We must bear in mind, that what we call income, is applicable to national defence only in so far as it gives the possessor a hold on the labour of the community. As long as the labourers exist, and the produce of the ground can be commanded by persons living in the country, their labour will be called forth, and the same scope will remain for all that taxation which is subservient to military objects. The *wealth* of the country, according to the definition formerly given of it, must be diminished; and the prices of many articles of consumption may fall. A diminution of the amount of the taxes consequently takes place: but this must be attended with a reduction in the price of those articles by which the public service is supported. The expenditure and the revenue still bear the same mutual proportion.

These doctrines will apply to every case in which the navy and army consume the produce of our own country: and if it be considered that our own country produces the most essential articles, and that our colonial possessions furnish a variety of those which we usually procured from other quarters, it will be found that we possess, in an ample degree, the means of maintaining our fleets and armies, independently of foreign commerce. Some apprehensions which were once entertained regarding the possibility of procuring naval stores, when cut off from the commerce of the continent, seem now to have vanished, in consequence of the inquiries that have been made on the subject.

We shall therefore pass from financial considerations to another topic highly worthy of attention, namely, the reply which Mr. Spence has given to those who consider foreign commerce as an indispensible nursery for seamen. It seems perfectly clear, that, whatever has been the origin of our naval power, its duration is independent of a commercial navigation, because young men can be trained on board ships of war to every naval operation, with even greater advantage than in merchant vessels. This is an argument to which no rational reply has yet been given.

I hope that no apology is necessary for having dwelt so much on the subject of war. That ambition, whether of individuals or of nations, from which it most frequently arises, is a mean and a vicious sentiment. But the strictest system of self-defence is the only species of war to which I have directed any portion of your attention; and independently even of this, we may consider the points now adverted to, as affording a profitable subject for discussion, which have, in some instances, been too much overlooked; and in others, subjected to too hasty decision.

APPENDIX.

In a Letter to the President and Members of the Literary and Philosophical Society of Manchester.

(Read October 4, 1811.)

GENTLEMEN,

SINCE I had the honour of reading the preceding observations in your hearing, some further discussions on the subject have been presented to the public. Considerable additional light has been thrown on it in Mr. Chalmers's inquiry into the nature and extent of national resources. The observations of that author might produce an extensive effect on the general mind, if they were luminously prosecuted in detail, and patiently defended from common objections.

A second pamphlet also has been published by Mr. Spence, in which he has not corrected his opinions so carefully as might have been expected; and his errors have met with pointed reprobation in another article devoted to the subject in the Edinburgh Review.

The author of that article, however, has made assertions which require to be carefully weighed before we can give them our assent. He, no doubt, justly accuses Mr. Spence of assuming erroneous data for the foundation of his reasonings; but he is not successful in refuting his leading conclusion, that Britain is independent of commerce. If we wish to estimate the truth of that conclusion, and the degree of importance that ought to be attached to it, we must beware of mistaking the point at issue, by allowing the meaning of the word *independence* to be insensibly shifted. We must enquire what was the common impression on the subject previous to the discussion, and what is the result to which that discussion has led.

The fixed and almost universal impression was, that the moment foreign commerce is shut up, the power of Britain must be annihilated. This apprehension has certainly been removed. The loss of foreign commerce is indeed acknowledged to be productive of privations and sacrifices; but these by no means amount to national ruin. A nation unwilling to submit to sacrifices, is always pronounced unworthy of independence. Every war implies sacrifices to which we are not

subjected during peace. The hardships, dangers, and losses, inseparable from military service, need not be recounted; but no definition of national independence has ever yet been received, which implies that this independence is lost as soon as a nation is obliged to go to war. An attachment to the cause in which the soldier perishes, often consoles the affliction of surviving friends. In the same manner, if we resign our foreign commerce in an honourable cause, the immediate sufferers, if their patriotism is ardent, will receive, in the general advantages secured to their country, some consolation for their personal distresses; and the nation at large may, without any want of sympathy for the disappointed merchant, reckon such evils necessary for the public interest. The merits of every particular case are a fair subject of inquiry: but they are foreign to the present argument. It may however be laid down as a very moderate assumption, that the losses of the merchant ought to be as easily consoled, as the calamity often sustained in the death of valued friends.

The disadvantages arising from the loss of foreign commerce, do not bear so close an analogy to the common calamities of war, as they do to evils of much inferior magnitude.

They resemble those which have sometimes resulted from extensive commercial speculations turning unfortunately out. Private losses of the same kind arise even from occurrences which are productive of essential gain to the country. Working people are put to as much inconvenience by the change of fashion in a particular luxury, or even by the invention of machinery which supersedes a portion of their labour, as they are by the decay of foreign commerce. They are equally obliged in each of these cases to enter on a species of labour to which their habits are not adapted. When peace is concluded, and foreign commerce restored, it is not unlikely that this restoration will bring with it inconveniences exactly similar to those which follow its temporary departure.

The alarms of merchants and manufacturers are not confined to such occurrences as the loss of foreign connections, but are sometimes loudly heard when changes are apprehended which are undoubtedly beneficial to the public. Of this sort was the alarm taken by the woollen manufacturers of Yorkshire, when it was proposed to give greater freedom to the trade and manufactures of Ireland. With the same justice might the proprietors of West Indian plantations deprecate the

taking of any sugar islands from the enemy, as an event that must overstock the market of sugar.

Some consolations, of a commercial nature, mentioned by Mr. Spence, are greatly underrated by the reviewer. When the large manufacturing establishments by which the foreign market was supplied are reduced, a part of them is acknowledged to be retained. That part is not sufficiently proved to be contemptible, because in a comparative point of view, we may attach to it the epithet *puny*. It serves the purposes of our own consumption, and it gives employment to a part of the labouring population. Nor when establishments altogether new are formed at home to supply us with articles which we formerly procured at a lower rate from abroad, ought their awkwardness and the inferiority of their produce to be treated with unqualified contempt: especially when we consider, that lately the general impression was, that the loss of foreign commerce brought along with it the ruin of every commercial and manufacturing establishment; and that no ability could exist of forming any new establishment, even the most insignificant. The inferiority of the article may imply no sacrifice which is not compensated by the advantages which the

insulation of our interests secures to the country. Even this change in the direction of industry, has in itself a chance of securing some permanent advantages. Foreign manufactures may, in some instances, owe their advantages to their previous establishment. The removal of a manufacture to another local situation, is, in general, a process too tedious and expensive to be prudently undertaken by private individuals. This circumstance often prevents the establishment of manufactures which might ultimately prove beneficial. On this principle, it is probable that manufactures might, in a case of necessity, be introduced, in which no individuals would otherwise have had the hardiness to engage. Our exclusion from the foreign market, would thus ultimately add to our permanent domestic resources. But it is most important of all to recollect, that such establishments will exactly suffice to give employment to that part of the labouring population which the reduction of our former manufacturing establishments throws idle.

The merits of this whole question, and of some others closely connected with it, deserve a more full discussion than has yet been given to them. When we have been emancipated from the slavery of unfounded apprehensions,

we ought not to be insensible to the hardships which really attach to the loss of foreign commerce. There is, no doubt, room for devising expedients by which the pressure of these might be considerably alleviated.

Much improvement may also be made in the art of disseminating more widely those principles of political œconomy which are established on sound reasoning. Those literati who have ready access to the press, and leisure for instructing the public, should spare no exertion to combat error in all the channels in which it flows. No periodical publication, however trifling, should be suffered to soothe the prejudices of the ill-informed, without an offer being made to exhibit the antidote along with the poison. The difficulty of convincing the public of truths which they have not been in the habit of believing, should not give rise to elegiac lamentations, rude reproaches, or contemptuous neglect, but be viewed as a fact which furnishes a motive to patient exertion, and which, when minutely studied in its various aspects, will suggest the means of conveying truth more successfully. Great care ought to be taken to avoid confounding those questions which are agitated by the political parties of the day, with any particular argument with which such questions

are not necessarily connected. Those who believe that all existing misfortunes have arisen from mismanagement on the part of government, should, whenever it is possible, proceed on the same data with those who believe that the wisdom of government has averted the most dreadful calamities, and secured to us all the advantages which we enjoy. The questions of parties are often indeed highly important; but by avoiding their influence, where it is not strictly legitimate, we shall render our discussions the more candid, and make some advances, in giving to such questions a greater simplicity. When political adversaries are mutually deprived of their fallacious arguments, they will come to a better understanding on the remaining points of difference; and if their spirit is manly, they will make gradual and cordial approaches. The triumph of reason is equally grateful to an ingenuous man, when his own fallacies are refuted, as when those of his antagonist are exposed,

I have the honour to be,
Gentlemen,
Your obedient and humble servant,
H. D.

Lassoddie, by Kelty-bridge,
12th Jan. 1811.

REMARKS

ON THE USE AND ORIGIN OF

FIGURATIVE LANGUAGE.

BY THE REV. WILLIAM JOHNS.

(Read October 21, 1808.)

Quanquam hoc videtur fortasse cuipiam durius, tamen audeamus imitari Stoicos, qui studiose exquirunt, *unde verba sunt ducta.*—CICERO.

A CORRECT notion of the origin and use of *Figurative Language* will greatly assist us in discovering the principles according to which language has been formed and improved. Though much light has been thrown on the subject of the formation of language by modern critics, and especially by Mr. Horne Tooke, yet I cannot help being of opinion that room is left for further discoveries; and under this impression, I offer the following theory to the candid consideration of the society.

This essay has a two-fold object, and naturally divides itself into two parts; but, as far as relates to my present purpose, the first is only subservient to the second.

The first shews the *nature* and *use* of figurative language; the second traces it to its source, and deduces from it some properties of language hitherto, I believe, but little known.

I. The nature of figure is generally understood, and has been critically explained by writers on rhetoric, both ancient and modern. Figure is a change of words, either from their original meaning, or from their most usual and commonly received acceptation. This change, according to the different circumstances under which it was used, was denominated by the ancient writers on oratory, (whom the moderns have copied) *tropus*, *figura*, *metaphora*, *translatio*, and many others; all which terms imply a change in the use of certain words in a discourse, and a turning of them from what may be called their *original* and *proper* meaning. Thus when we say, that the Roman empire flourished under Augustus, that splendid victories were gained, and that the arts were cultivated—the words *flourished*, *splendid*, and *cultivated*, are obviously metaphorical, being transferred from their proper acceptation to one that is merely analogical, and one only of real or fancied resemblance.

Of the different sorts of words into which speech has been usually divided, the substantive, the adjective and the verb—and likewise the adverb, when derived from any of these three, are obviously capable of suffering the foregoing transformation. Of the other parts of speech we at present affirm nothing. It is scarcely necessary here to exhibit instances of each, as language, whether prose or verse, luxuriantly abounds in them—indeed much more so than is generally imagined. A few promiscuous examples, nevertheless, may not be deemed improper.

Patet isti *janua* letho.—VIRG.

Dulcia linquimus arva.—*Id.*

Corydon *ardebat* Alexim.—*Id.*

O et *columen* et *dulce decus* meum.—HOR.

Tu, cùm te de *curriculo* petitionis deflexisses, &c.
CIC. pro Muren.

Sed tamen, Servi, quam te *securim* putas *injecisse* petitioni tuæ, &c. *Id.*

Smit with the love of sacred song.—MILT.

The *full* blazing sun,
Which now *sat* high in his meridian *tower*.—*Id.*

And the moon
Riding in her highest *noon*.—*Id.*

Now is the *winter* of our *discontent*,
Made *glorious summer* by this *sun* of York,
And all the *clouds* that *lowered* upon our *house*,
In the deep *bosom* of the ocean *buried*.—SHAKESPEARE.

II. This part of my subject being sufficiently explained for my present purpose, I shall now proceed to shew for what reasons and under what circumstances metaphorical or figurative language originated; what purposes it serves; and how, in the progress of language from infancy to maturity, words assume, renounce, and re-assume a figurative meaning.

The most judicious critics truly ascribe the origin of figurative language to necessity. This is expressly stated by Cicero in his book de Oratore.

"Tertius ille modus transferrendi verbi late patet, quem necessitas genuit, inopia coacta et angustiis."—*Lib.* III. 55.

We use figurative language, according to Quinctilian, "aut quia necesse est, aut quia significantius est, aut quia decentius." In explaining the case of necessity, he adds: "Necessitate rustici dicunt *gemmam* in vitibus; quid enim dicerent aliud? et *sitire* segetes, et fructus *laborare*. Necessitate nos, *durum hominem*, aut *asperum*. Non enim proprium erat quod daremus his affectionibus nomen."—*Lib.* VIII. *Cap.* VI.

Dr. Blair has expressed nearly the same sentiments in his Lectures on Rhetoric: "Tropes of this kind abound in all languages,

and are plainly owing to the want of proper words." He errs, however, in my opinion, in adding soon after, "that necessity is not the principal source of this form of speech; but that tropes have arisen more frequently, and spread themselves wider from the influence which imagination possesses over language." This indeed may be true in regard to those figurative expressions for which proper ones might readily be found, which spring only from a wanton search after analogies, and which add neither force nor justness to a sentiment; but it can by no means be true concerning those tropes of which the great body of a language consists, and without the assistance of which we can scarcely utter a sentence. It will appear too, from the manner in which I shall endeavour to trace the rise and origin of figurative diction, that we do not owe much of it to the influence of imagination, but that the want of proper words of sufficient force and significance, has obliged men to supply the deficiency in the best manner they were able.

I regard the appropriating of distinct names to sensible objects as the first step in the formation of language; and, therefore, I consider nouns as the basis of the whole superstructure. Without nouns there cannot, in

the nature of things, be any communication of ideas. External objects are the only things with which the senses can be impressed; they are the first things with which men could become acquainted, and they must be regarded as the primary objects of knowledge and attention. The invention of those elements of speech, which grammarians have denominated nouns, was at once the most obvious and the most necessary. I shall therefore consider myself authorized to assume it as a fact, that the appropriation of articulate sounds to specify different sensible objects, is the groundwork of all language.

Dr. Blair, however, is of opinion, "that those e clamations, which by grammarians are called *interjections*, uttered in a strong and passionate manner, were beyond doubt the elements of speech." But after these inarticulate cries, he gives the next place to nouns.

If by interjections Dr. Blair meant only inarticulate cries, we observe, that they are not to be deemed any legitimate part of language; but if those broken fragments of speech be meant, to which grammarians may have sometimes given the appellation of interjections, it is sufficiently evident that these

must be posterior to those words from which they are derived.

But though nouns compose the primary and most necessary part of language, men would soon discover, that it is absolutely impossible to denote every individual object by a distinct name. This indeed is a task of such extent and difficulty, that the life and faculties of man are not equal to it. Necessity, therefore, drove men to devise some means to accomplish the end of mutual communication, without attempting a hopeless labour.

One of these contrivances, and one of very extensive application, is transferring the name of an object already known in language to an unnamed object; in doing which, men were guided by analogies more or less obvious, more or less remote. Upon this principle, death would be called *sleep;* a governor, *head;* ignorance, *darkness;* knowledge, *light,* &c. Remote as we are from the original formation of languages, there are but a few instances *comparatively* in which we can trace the names of objects to their primary source, because the analogy followed has no certain or determinate laws or restrictions, and because the disuse of most words in their primary signification has been of so long continuance, that scarcely any trace or vestige is left to form a

clue; yet the number of instances is sufficient to verify the fact, and fully to shew the nature of the modification which innumerable words must have undergone.

Generally in all languages, in the English, at least, in its present state, men do not absolutely invent *new* names (as *quinbus flestrin*, in the voyage to Lilliput) to denote new objects or things, but either *compound old ones*, or use old names in a *new* or *transferred meaning*. Thus at the invention of the machine which turns a spit, it was called *jack*, in compliment to the former operator. The contrivance to change the level of a vessel in a canal is called a *lock*, from its confining the vessel, by an analogy rather remote. When an artificer in wood-work confines his object to one species of labour, e. g. making carts or wheels, he is denominated *cartwright* or *wheelwright*. There are other compositions in the language, in great numbers, less apparent, but not less real: as in the words *Godhead*, *goodness*, *saintship*, *gaily*, *preferment*, and others; the latter syllable was originally a word or part of a word, in composition with *God*, *good*, &c. There would have been many more contrivances of this nature in our language, if it had not borrowed so copiously from other

languages, in which such contrivances had already taken place.

The names, and expressions, which have been applied to the mind, its faculties and operations, are transferred from external objects and the corporeal senses. Spirit means *wind* or *breath.* Were I sufficiently acquainted with the Anglo-Saxon, I do not doubt but mind and soul might be accounted for in the same manner. Who does not at once see the transferred use of the words *action, passion, affection, understanding, perception, recollection,* and many more, when used in reference to the mind? What is more common than to say, *I see, I take you, I assent, I consent, I refuse, I reject,* &c. &c.? Though we cannot, at this distance, ascend to the primary objects to which these words were applied, we can, at least, retrace them so far as to prove our object—that after men had proceeded to a certain extent to give names to sensible objects, their next step was to apply these names figuratively to other objects, according to a certain analogy, real or supposed.

All intellectual ideas are expressed by the names of sensible objects, from a supposed analogy or resemblance, the best the circumstances of the case were supposed to admit of.

Thus a connection, in a long process of reasoning, is called a *link* in the *chain* of reasoning. But not only the words *link* and *chain* are obviously used in a transferred sense, but likewise *connection* and *process*, (which seemingly express intellectual, as aptly as sensible ideas) are in the same predicament, because they are traced at once to *connecto, to tie together;* and *procedo, to go forward. Reason*, too, I am confident, is a word of the same stamp, though I cannot now recur to what grammarians call its *etymon, i. e.* its *true* or proper meaning. Suppose, however, we allow the word *reason* to mean, in its proper acceptation, a *faculty of the mind*, yet, in the progress of language, it becomes to be used in a transferred meaning, in several instances. By a process, to be hereafter explained, it becomes a verb, *I reason*. It stands not only for the act, but also for the thing acted or done, *i. e.* the *thing reasoned*, the conclusion. It stands likewise for *cause* or *motive*, as in the following expressions: "the sea arose *by reason* of a great wind;" and, "they received the men *by reason* of their victuals." It may sustain other meanings; but it is not my object to comprize them all.

In the language of rude and savage nations, the number of words, in a proper sense,

(*extra figuram*) is small; but words used figuratively, or in a transferred sense, are endless. With them, war is a *fire, fire and sword, a tempest, the red axe,* according to the view which they are analogically led to take of it; peace is an *olive-branch;* mourning is *sack-cloth and ashes;* a dwelling or habitation is a *seat;* and so in instances almost innumerable, if they were diligently sought. Poverty of language necessarily requires the use of figurative speech. It will consequently be found, that, in the progress of all languages, in proportion as sensible objects are destitute of names, figurative, or borrowed names, will be used. Though we are greatly removed at present from this state of language, yet some traces of it may still be discovered, and something analogous to it is still used. The word canon is used for a *law* or *rule,* and, with an immaterial difference in the spelling, for *a large gun;* house is both a *building* and *a family.* The words *place, post, rank,* and many others, tedious to enumerate, have the same variety of signification. If language is now under the necessity of having recourse to such awkward expedients, we may well imagine that that necessity was much more severe in a ruder state.

Men, however, must have constantly endea-

voured to free language from these shackles. From the evident inconvenience and confusion arising from a multiplicity of objects being denoted by one name, facility and clearness of communication required that they should, as speedily as possible, appropriate distinct names to every individual object, as far as practicable. Thus, if the word *knave* meant *primarily* a *labouring man* or a *servant*, but came gradually to signify a man basely dishonest, from the prevailing character of that class of people in former times, this would clearly be a transferred or figurative meaning; and if the word *knave* happened in time to be entirely discontinued in its primary or proper meaning, the figurative one would appear and come to be considered as its proper meaning. For which reason, every word thus appropriated, became, as it were, divested of its figurative meaning, and had a proper character of its own; so that it could not be applied to any other object, not even to that for which it originally stood, without again sustaining a figurative character. Knave, in the sense of *servant*, is now an improper meaning, and the transferred sense of a *man basely dishonest*, must be regarded as its *proper*, extrafigurative acceptation.

To exemplify this by another instance: If

we suppose that the word *pax* in Latin, originally, in its proper sense, meant *agreement*, and *peace* in a figurative one, from the circumstance that an agreement precedes peace; but that, in process of time, another word, *pactum*, came to be used for agreement, and *pax* was confined to the meaning peace; we imagine it will be readily granted, that *pax* again becomes figurative, when it is made to denote any thing besides a termination of hostilities, even if it were made to express that very idea which it was originally used to convey.

We asserted above, that the first language of men must have been *nouns*, or the *names of sensible objects*,* because without these no verbal communication can be attempted, or even imagined. It is certainly possible for the communication of ideas to be carried on by means of nouns only, when duly assisted by the gestures and actions of the persons speaking, and by indicial references, i. e. the pointing to different objects remarkable for the quality or action meant to be expressed. It is a circumstance of frequent occurrence among rude or savage tribes, to have recourse to actions significant of the meaning intended.

* Gen. ii. 20. And Adam gave names to all cattle, and to the fowl of the air, and to every beast of the field.

Thus the expression of *burying the axe*, used among the American Indians, for *making peace*, may be regarded as a sort of historical record of the circumstance being at some former period actually transacted. We have an account of something very similar to this in the writings of the Jewish prophets.

But such a mode of communication is extremely imperfect and deficient, and could be but of short continuance. Necessity plainly required, that words should be used under the character which distinguish verbs and adjectives as parts of language. No communication, purely *verbal*, can possibly take place without the use of *adjectives* and *verbs*, in addition to nouns, or the names of things.

Here, then, a most important question immediately occurs—What is the origin of these words? Did men absolutely invent names, *de novo*, for qualities and actions? For instance, when the word *fly*, for the action of flying, was first used as a verb, was it a verb in its first utterance, and at the same time an entirely new word, or was it the appropriation, in a certain way, and according to certain principles, of the name of the insect *fly*, to express the action which it very frequently performs? To me, the latter appears to have been the case, in all instances, in the formation of verbs;

and, consequently, every verb, as far as regards its origin, is to be considered as a noun, in a figurative or transferred sense. It may, indeed, be termed its proper sense, as soon as its use as a noun is discontinued, or when it is accompanied with circumstances plainly characteristic of a verb, (which circumstances are hereafter to be specified); but I cannot help regarding all verbs in their *origin*, as nouns in a transferred or transformed sense.

Indeed, not only verbs, but all words, under whatsoever division of the parts of speech they may be classed, were in their original or primitive state, the names of sensible objects.

When we have occasion to express any new or very unusual action, we never coin a new word, properly speaking, (as *quinbus* or *flestrin*, as before) but we either apply to some existing noun the concomitants of verbs, as *it* snows, *I* water, *to* place, *they* murmur*ed*, &c.; or we add to nouns fragments of verbs previously in use, as *electrify*, from ἠλεκτρον and *fio; scandalize*, from *scandal* and ιζω, a termination borrowed from the Greek.

It is a curious speculation to investigate what was the actual process in the distribution and translation of nouns into the other parts of speech, *i. e.* into verbs, adjectives, pronouns, and prepositions: as to the adverb, it is

included in the rest. A clear insight into the nature and genius of language, will, I am persuaded, determine in favour of the hypothesis, that all the grammatical divisions of speech are, in their origin, resolvable into modifications of nouns.

A noun is converted into a verb, so as to answer every practical purpose, whenever by means of gesture, by peculiar utterance or intonation, or by position in a sentence, it is made to communicate the notion of *motion* or *action.*

I beg leave to introduce here a short extract from Mr. Jones's excellent Greek Grammar. In explaining the *origin of verbs,* he says, (page 132. 2d *edit.*) "We acquire the idea of *action,* by reflecting on ourselves, or observing others, in certain circumstances; and the most simple way which nature could at first suggest of expressing these ideas, was to combine the name of the person or thing which acts, with the person or thing acted upon. Thus, οινος and εγω joined and abbreviated, is οινοω; and this term would be sufficient to express *I drink wine,* though originally it meant only *wine I;* association supplying to the speaker and the person addressed, the intermediate notion of *drinking.*"

From this explanation of the origin of verbs, he draws the following conclusion: "Verbs were originally the names of things, and received their character as verbs from association." (p. 133.) What Mr. Jones calls *association*, I call *translation*, or *change of meaning*, because it appears to me to approach as near as possible to what the rhetoricians have so denominated.

It is scarcely necessary to exemplify how actions can be indicated by gestures; for who can be ignorant of the *index*, ☞; of the sign of silence, *the finger on the lip*, &c.? In a poor language it is well known how much gesture assists communication.

If in the primitive state of a language a person wished to inform another that a certain man (Thomas) had escaped him, (John) by running or starting swiftly from him, he would, probably, for this purpose, select the name of some well-known animal, remarkable for the habitual practice of the action he wished to express—we will suppose a *fly*—and with appropriate gestures, express himself thus: Thomas fly John. This is probably a true history of the origin of the English verb to *fly*. Thus, by peculiarity of utterance, or greater intonation of voice, the name of any object would be made to represent the action

of property for which it was most remarkable.

Lastly, it is easy to imagine how position would change a noun into a verb, because it is now practised very extensively in our own language; as is well exemplified in the following expressions: Flag the floor; floor the house; John, load the horse. Here *flag*, *floor*, *load*, are verbs, merely by the position, assisted, perhaps, by the utterance and intonation of voice. Sometimes it is not easy to determine whether a word is a noun or a verb, as in the sentence, "John is prone to *love*."—But I forbear to produce examples.

Adjectives were originally formed from nouns, in nearly the same manner as verbs. A word closely joined to another in position and utterance, would signify the addition of its own most prominent attributes or properties. When, for instance, it was necessary to call any man *strong*, or *swift*, the name of the animals most distinguished for those qualities, was joined to his name. Thus *lion-man*, would obviously enough express the notion of a *bold, strong, and courageous* man; and *swallow*—or *swift man*, signify a man that can move with great rapidity. The adjectiving * of nouns to nouns, in this manner, is

* A word used by Horne Tooke.

of extensive use in English, as in the following expressions, and numerous other similar ones: *sea-water, dragon-fly, master-key, day-light,* &c. &c. Mr. Horne Tooke has very fully explained this part of the subject.

The consideration of *prepositions* and *conjunctions,* I think, may be safely omitted, as the author just mentioned has demonstratively proved them to be the fragments of other parts of speech, especially of verbs, the developement of which has been already attempted in this essay. I do not however judge it amiss to add the following attestation of Mr. Kirwan, to the truth of the theory:

"The celebrated Mr. Horne Tooke, in a very subtle and ingenious work, has shewn that even those particles that denote the relation of objects, or of sentences, with each other, originated from circumstances apparent to the senses."—*Kirwan's Logic,* v. I. p. 12.

In regard to the verb, substantive, and the pronouns, I do not well see how they can be traced to their sources, by the method of the formation of language now proposed; nor indeed am I acquainted with any satisfactory theory of their formation. This, however, is by no means sufficient to overpower the evidence which has been adduced. It is only a negative kind of objection. At best it is

only an argument ex ignorantia. No one, I suppose, will presume to say, that the pronouns, and verb substantive, were at a certain stage, in every language, coined by a council of philologers, entirely de novo, from the chaotic mass of unformed sounds. This surely is wholly inadmissible. Their formation according to the method here supposed, is as probable as any other, if not more so. They are words, which in comparison with the other words of which speech is composed, are exceedingly few in number, and for that reason, though abundantly useful, are the least necessary for abridging the process of communication, and perhaps were of late invention. By the same process, by which proper names became appellatives, might names of very frequent use, as nominatives to verbs, become pronouns. Hence, it appears to me highly probable, that pronouns are nothing but fragments of some names or words of very frequent recurrence, which men gradually learned to substitute for the *proper* names of the persons or things frequently repeated, and by which they obviated a very evident and awkward inconvenience.

If it be admitted, conformably to the theory above stated, that the first language of men was nouns, or the names of sensible objects,

or in other words, that nouns form the elements of all language, and that these names, appropriated to sensible objects, were *translated*, (to use the Latin term) not only to signify other sensible objects of similar properties or form, but likewise to express qualities, actions, &c. as already pointed out, it must follow of course, that all nouns, except those used in a proper meaning, (*i. e.* originally appropriated to sensible objects) that all adjectives and verbs, without exception, and, consequently, all the words derived from them, are to be viewed originally as figurative language; for the very essence of figurative language consists in changing words from their *originally proper* meaning.

As the purposes of communication thus required words to be constantly used in a figurative sense, *i. e.* either as figurative nouns, or as adjectives and verbs, it would happen in various instances, from different causes,* that words came to be disused in a literal meaning, and only the figurative use to be continued. From the time of this change, their commonly received meaning must be regarded as the proper one, and every other meaning, even that in which they were originally used, as

* Those causes, whatever they be, which effect the constant flux of language.

figurative. This might be exemplified in numerous instances, but the selection of a few will perhaps sufficiently explain my position.

The expression, *I understand you*, in its originally literal meaning, is disused, and that should be considered as its proper meaning in which it is commonly used; and if any person should now use it in the sense of *I bear you*, or *I stand under you*, it would plainly be considered as metaphorical.

The word *extravagant*, must once, in its proper meaning, have signified *wandering beyond the ordinary, or allowed limits*. But having been long disused in this sense, its *proper* meaning now is, *wasteful and immoderate in expence*, &c. When, therefore, we say of a man that he is an extravagant liver, or extravagant eater, we use the word in a proper meaning; but we should express ourselves in a manner highly figurative, however agreeably to the originally unfigurative meaning of the word, if we said that a prisoner is extravagant when he breaks his prison; that a river is extravagant when it overflows its banks *; or a horse if he broke from his pasture.

* ——————— vagus et sinistra
Labitur ripa (Jove non probante) uxorius amnis.—Hor.

The analogies by which words may be transferred from a proper meaning to a figurative one, are, in general, so obvious, that there is no difficulty in applying a word to a new meaning, nor in understanding the application. Words, thus affected, are constantly used by the speaker, and equally well understood by his hearers. In many, perhaps in most instances, the analogies are so obvious, that we are not aware, except by reflecting on the subject, that the words used are diverted from their proper extra-figurative signification. Thus, for example, the expressions *go on, stop,* though in their proper meaning they appear to be applicable to some kind of motion, *e. g. walking;* yet they are as readily applied to the action of *speaking, reading, writing,* and a variety of other actions, and are as easily understood.

This is the natural, and indeed the necessary progress of the creation of words in the formation of a language. Were new sounds to be embodied on every fresh occasion, were it allowable on no emergency to adapt an old word to a new meaning, language would immediately become more unwieldy than the armour of Goliah. As speech is now managed, a speaker has no occasion to be at a loss for the want of a word to express his

ideas, because he has the choice of analogies to an indefinite degree; and he is aware that the person or persons whom he addresses, are in the daily habit of deciphering the meaning of words by such analogies, and that he is in much greater danger of proving deficient in invention, than his auditors in comprehension. This habit of developing the meaning of figurative speech, we do not possess in any great perfection, except in our vernacular tongue; or, at least, it is perfect, or otherwise, in proportion to our acquaintance with any particular language. And hence the fact is easily accounted for, that, in the early progress of learning any language, we are frequently unable, by the mere help of a dictionary, satisfactorily to develope the meaning of an author. In every language, for various reasons, the mode of metaphorizing, or using figurative language, is, in a greater or less degree, characteristic and peculiar. And hence likewise arises the difficulty of using a foreign dialect with propriety, either in discourse or writing, because we are in continual danger of departing from the idiomatical mode of metaphorizing customary in that dialect.

The evidence of the above theory, if it may aspire to be so denominated, appears more or

less as we advance in developing and explaining it; and if its reasonableness have not already appeared, it will be to little purpose to seek for accessory or collateral proofs. The writer, however, would justly be chargeable with want of respect to this society, if not of deference to his own opinion, if he omitted some additional remarks which seem further to confirm what he has advanced.

Most writers on rhetoric have been sufficiently aware of the manner in which words assume a metaphorical attitude, as may be inferred from the short extracts which were introduced at the commencement. In confirmation of which change of words from a proper to a metaphorical meaning, the following sentence may be quoted from Quintilian's Oratorical Institutes: "Verborum vero figuræ et mutatæ sunt semper, et utcunque valuit consuetudo, mutantur. Itaque si antiquum sermonem nostro comparemus, pene jam quicquid loquimur, figura est."* There is a passage in Dr. Blair's Lectures on Rhetoric † which seems still nearer to coincide with what has been suggested in the foregoing pages. "In every language," says he, "there are a multitude of words, which, though they were figurative in their first application to certain

* Quintil. B. IX. c. III.—Gibson. † Lect. XIV.

objects, yet by long use, lose that figurative power wholly, and come to be considered as simple or literal expressions."

These authors, however, do not seem to have been aware, that words, universally, except those originally appropriated to sensible objects, were *sometime*, and at *some given point* in the progress of language, figurative; that by common and exclusive application to certain objects, figurative words become proper, and that they become again liable to the same laws of figurative application to which words are liable in their originally proper state.

That this however is actually the case, may be further shewn by an appeal to the progress and state of those languages which are best known. We shall discover that a great proportion of every language consists of words, which, however they may be regarded now, were once metaphorical. Let us examine whatever author we please, in prose or verse, we shall not fail to be convinced of this fact. I shall here beg leave to introduce a few instances, merely by way of example.

Spirit, signifying breath, or life, or spiritual existence, is only the word *spiritus*, wind, used metaphorically. Its appropriation, however,

to that meaning, is so *common*, that it may be, and is generally, deemed extrafigurative. *Ache* was originally an exclamation expressive of pain; by metaphorical transformation, it came, in process of time, to signify pain itself. The verb *to bite*, besides its common or proper meaning, has a metaphorical one in the following not uncommon expression, *the biter is bit*. It formerly likewise had another metaphorical meaning, still preserved in the compound *back-bite*, which signifies to speak ill of a man behind his back.

If we but open a system of geography, and read but the definitions, we shall immediately discover the method of metaphorizing which I have been endeavouring to explain. An isthmus is a *neck* or *tongue* of land, which *joins* a peninsula to a continent. A gulf or bay is an *arm* of the sea which *runs* or *stretches* into the land. A cape is a point or *nose* of land which *stretches* out into the sea. In these definitions, the use of the words *neck*, *tongue*, *joins*, *arm*, *runs*, *nose*, *stretches*, sufficiently corroborates the foregoing obse vations.

It is generally acknowledged that the Greek language is one of the most copious with which we are acquainted, and yet it is well known that its primitives are comparatively very few. These two facts, seemingly incon-

sistent, are in some measure accounted for by the scheme of the formation and progress above developed, and especially by the theory of metaphorizing proposed. These, together with the great facility of composition which this language enjoys in an indefinite degree, will sufficiently account for its wonderful copiousness.

The languages of barbarous nations, and their modes of speech, as represented to us in the fragments which are occasionally given by travellers and navigators, tend greatly to corroborate the foregoing observations. These fragments, however, it is not in my power at present to collect, and I only refer to them in general as sources of proof with which few scholars can be unacquainted.

The invention of hieroglyphic writing, which took place at a very remote period, affords a further proof and illustration of the foregoing remarks. The symbol which stood for any object, it is highly probable was only a picture of the name which in spoken language, by a metaphor, represented that object. Thus, if imprudence was expressed in hieroglyphic writing by the picture of a fly, for what other reason could it be, than because imprudence was expressed metaphorically in

oral language by the name of that little animal?

Words deriving their origin from a metaphorical source, are, without doubt, much more numerous than we are generally aware of. The names of all qualities and properties were, when first applied to express those qualities, used in a figurative meaning; and to this class must be added all the names of ideas which are denominated intellectual, as has been stated in the foregoing pages. The words *round, square, hard, soft, high, low,* and all similar ones, it is highly probable, (not to use stronger language) are of this description; but the words *affection, passion, understanding, spirit, inspiration, perception, invention, motive, habit,* with many hundred others, are so evidently in this predicament, that no reasonable doubt can be entertained on the subject.

Language is in a state of constant flux. Words, in the progress of speech, are continually undergoing various and important changes. These changes are beautifully described in Horace's Art of Poetry:

> Ut silvis folia privos mutantur in annos,
> Prima cadunt; ita verborum vetus interit ætas,
> Et juvenum ritu florent modo nata vigentque.

Mortalia cuncta peribunt ;
Nedum sermonum stet honos, et gratia vivax :
Multa renascentur quæ jam cecidere, cadentque,
Quæ nunc sunt in honore vocabula.

As the knowledge of things is acquired through the medium of words, it becomes highly necessary for us to become acquainted with the manner in which, in the progress of the developement of human reason, words have been applied to things, how they become the means of communicating thoughts and trains of ideas, and in what manner the structure of human speech has been built from the time of laying the first rough stone at the foundation, to the completion of an useful and ornamental edifice. Our knowledge of words can by no means be deemed perfect, except we are acquainted with their various modifications and changes. Though we actually learn a language and the different meanings of words in a manner very different from this—and that too in a manner fully adequate to all the useful purposes of life—yet in attempting to reduce language to its primary elements, and words to their original sources, we must be able clearly to see the whole course of their progress, their various windings and deflections, their compositions

that the argument respecting the measure of the force of a body in motion, was merely a dispute about terms, and that, though the force in question may be variously estimated, according to circumstances, it is most naturally and consistently expressed by the product arising from the mass being multiplied into its velocity.

Although scientific men have, for more than half a century, been generally satisfied on this question, it must nevertheless be acknowledged that considerable difficulties have occurred in the practical application of their measure of force; and, it is remarkable, that the measure which they have rejected, appears to have been first suggested to Hooke and Huygens, by their practical observations on the motion of pendulums, and was afterwards adopted by Smeaton, as a rule for the great operations in which he had so much experience.

It is much to be regretted that theory should appear to be at variance with practice, or that any ambiguity should remain on a question of such general application in mechanics.

It has often been asserted, indeed, that practical operations need not be affected by differences of opinion about the measure of force; for, there being no disputed facts, the

mere scientific explanation of the phenomena, it is said, can be of little importance to practical men.

On this point, however, Mr. Smeaton's observations merit particular attention. He says, in reference to mistaken notions about the measure of force, "that not only himself and other practical artists, but also some of the most approved writers, had been liable to fall into errors, in applying the doctrines of force to practical mechanics, by sometimes forgeting or neglecting the due regard which ought to be had to collateral circumstances. Some of these errors are not only very considerable in themselves, but also of great consequence to the public, as they tend greatly to mislead the practical artist in works that occur daily, and which require very great sums in their execution."*

Notwithstanding Mr. Smeaton's excellent experiments and observations on this subject, exhibiting much want of agreement between the theory usually given, and the practical results, the mechanical principles of force continue to be treated nearly as before; and, I believe, we are not without recent instances of errors similar to those which he has noticed.

* Philosophical Transactions, vol. 66. part 2d. p. 452

Mr. Atwood, in his Treatise on the rectilinear motion and rotation of Bodies, bestowed considerable attention on Mr. Smeaton's experiments and conclusions. He also observes, that Emerson, and other authors of merit, have been led into considerable errors, "by supposing the momentum of bodies to be as the quantity of matter into the velocity.* In that he agrees with Mr. Smeaton; but he afterwards concludes, that neither of the measures of force are capable of general application, and that for *one* class of the effects of force, we have no proper measure.

After discussing various examples of force, he proceeds as follows: "But the truth is, the principle (of permanent quantity) obtains not according to either of the measures, except in particular cases, which may be demonstrated as the other properties of forces are from the general laws or axioms.

"In the rectilinear motion of bodies, accelerated from quiescence, or retarded until they are at rest, the permanency of any given quantity of motion is demonstrated from the axioms, whether that motion be estimated by one measure or the other.

"In bodies which revolve round fixed axes,

* Treatise on Rectilinear and Rotatory Motion. Preface, p. 10.

the principle obtains, without exception, when the momentum is measured by the quantity of matter into the square of the velocity, but fails when measured by the quantity of matter into the velocity; a given quantity of motion thus estimated being alterable in any assigned ratio.

"In the communication of motion to bodies by collision, when the direction of the stroke passes through ths centre of gravity, the principle in question holds universally, according to the measure of the mass into its velocity, but fails when the momenta are estimated by the mass into the square of the velocity in every case, except when both bodies are perfectly elastic, or one perfectly elastic, and the other perfectly hard.

"Lastly, when motion is communicated to bodies by impact, the direction of which passes not through the centres of gravity, the quantity of motion communicated, whether estimated by one measure or the other, preserves neither equality nor any constant proportion to the quantity of motion impressed."*

These conclusions appear to be rather paradoxical, but they are neither new nor uncommon.

* Treatise on Rectil. and Rotat. Motion, p. 366—368.

It is true they have not been usually stated in the same terms: but I believe the same inferences strictly follow from the reasoning of many other good writers on this subject. If forces be mathematical quantities, we may reasonably enquire how it is that they are so indeterminate in relative magnitude?

If two given lines, angles, surfaces, or solids be equal, they are equal in whatever manner they may be applied, or however they may be measured. But if we have two given bodies, moving with velocities inversely as their masses, their forces, it would appear, are either equal or unequal, according as they may be classed under one or other of the above subdivisions of mechanical phenomena.

If the forces of two given bodies in motion are either equal or unequal, according to the purpose to which they may be applied, it would be very desirable to have a complete and accurate classification of all the phenomena of force, exhibiting the variations to which they may be subject; and we are so far indebted to Mr. Atwood, that he is, I believe, the only author who has attempted to make such an arrangement. But his arrangement is not complete, for he has omitted to include in it many important practical applications of force; such, for example, as the raising of a

body to a given height, where it is to be left at rest;—the driving of piles;—the overcoming of friction;—the grinding of corn;—the hammering and rolling of metals; and various other applications of force of a similar kind.

Mr. Atwood appears, however, to have been aware that the doctrines of force, as they are usually treated, could not be of much service in practice; for, a little farther on he observes, "It is not probable, that the theory of motion, however incontestible its principles may be, can afford much assistance to the practical mechanic; and there appears as little room to imagine, that any errors or misconceptions which may have been propagated concerning the effects of forces considered in a theoretical view, have at all impeded the due construction of useful machines, such as are impelled by the force of wind or water, by springs or any other kind of motive power. Machines of this sort, owe their origin and improvement to other sources: it is from long experience of repeated trials, errors, deliberations, corrections, continued through the lives of individuals, and by successive generations of them, that sciences, strictly called practical, derive their gradual advancement from feeble and aukward beginnings, to their most perfect state of excellence."*

* Treatise on Rectil. and Rotat. Motion, p. 281

But he has, in this instance, I apprehend, pressed his argument rather too far; and he is quite at variance with Mr. Smeaton, who has pointed out many inconsistencies in theoretical conclusions, which have been carried into practice with most injurious effects.*

It cannot be doubted, that ingenious men, of rare natural endowments, have, without any scientific aid, accomplished wonders in the invention and improvement of machinery. But how can it be supposed that these men could have derived no assistance from a clear and sound knowledge of the principles of

* See Philosophical Transactions, vol. 66. part 2d. p. 452, &c. and the following note, p. 454. "Belidore (Arch. Hydr.) greatly prefers the application of water to an undershot mill, instead of overshot; and attempts to demonstrate, that water, applied undershot, will do six times more execution than the same applied overshot. See vol. 1. p. 286. While Desaguliers, endeavouring to invalidate what had been advanced by Belidore, and greatly preferring an overshot to an undershot, says, (Annotations on Lecture 12. vol. 2. p. 532.) that from his own experience, "a well-made overshot mill, ground as much corn in the same time, with ten times less water;" so that betwixt Belidore and Desaguliers, here is a difference of no less than 60 to 1.—*Smeaton.*

Each of these authors has been considered by many as the best authority for practical men; and their various inconsistent rules have often been adopted, in the construction of expensive machines, in this country, as well as on the continent.

mechanics? Every new combination presented to their minds must have involved them in new and repeated labours to ascertain its effects; and these labours must have frequently terminated in a conviction that their time and pains had been wasted in examining old facts under new appearances. Such disappointments have sometimes served indeed rather to stimulate than to damp their zeal for making farther discoveries. But if a good theory in physical science be understood to comprehend a distinct arrangement of what is known on the subject; or if it furnish the means of applying the experience of one case so as to determine the result of another of the same kind, but different in degree, or under different circumstances; it cannot be questioned that such information must tend to shorten the labours, and smooth the path of the ingenious inventor; and still more valuable must it be to those whose task it is to distinguish the curious from the useful, and to carry into execution the real but not the fanciful improvements.

Neither does it appear that Mr. Atwood is supported in his opinion, by the history of useful discoveries in mechanics. If Huygens and Hooke had not been scientific as well as ingenious men, we might possibly have been

still ignorant of the properties of the balance regulated by springs. If Smeaton had not availed himself of just theory, as well as experiment, we might still have had to learn the principles by which we must be guided in applying water to the best advantage as a moving power. If a clear and strong understanding, and a mind richly stored with scientific attainments, had not been combined with wonderful fertility of invention, in the justly celebrated improver of the steam-engine; incalculable labour might still have been wasted in performing operations which are now accomplished with as much ease and regularity as the gentle motions of a time-piece.

But if it were even granted, that all these distinguished men might have attained their objects without the aid of theory; it must still be acknowledged, that to those who have to follow their steps, and to apply their inventions and improvements to various purposes, under various circumstances, it must be of essential importance to be free from perplexity in the principles by which they must be governed; and it is under this impression that I have been induced to state to this society some of the difficulties which have occurred to myself, in common, I believe, with many other practical

men, in the application of the prevailing doctrines of moving forces; in the hopes that others, better qualified for the task, may be prevailed upon to reconsider the subject, and remove the obscurities in which some parts of it appear to be involved.

I shall first briefly describe some particular cases where these difficulties occur, divesting them as much as possible of all complicated circumstances; and I shall be careful to state such facts only as will be readily admitted by any one moderately acquainted with the subject. I will then quote, from approved writers on mechanics, such observations as appear to have been given in explanation of the points in question, accompanied with some remarks which they seem to require; and I shall conclude, by venturing to offer some farther explanations, which appear to me to be capable of general application in mechanics.

Examples of Force producing Motion in Bodies from a State of Rest.

1. If two balls, A and B, (figure 1.) whose masses are as 1 to 4, be suspended like pendulums; and if they be set in motion by two equal weights, C and D, acting on them by means of the bent levers, E and F, whose

fulcra are fixed, and whose perpendicular arms are equal, but the length of the horizontal arm of F, twice the length of the corresponding arm of E. If C descends through the space S, D will descend through an equal space in the same time; and by these equal forces in equal times, A will have acquired exactly twice the velocity of B. Now if these effects are to be measured by the products of the masses into their velocities, D produces twice the effect of C, although their forces are precisely equal.

In this and the following cases, the mass of the lever, &c. is supposed to be indefinitely small, when compared with that of the ball which it moves.

2. If we suppose two balls, *m* and *n*, (fig. 2.) whose masses are as 1 to 2, to be suspended as in the last case, and put in motion by the pressure of the atmosphere on the pistons P and Q acting upon *m* and *n*, by means of the levers G I and A B; A F being equal to B F, but G H=2 H I, and the area of the cylinder E twice that of C; supposing these cylinders and the fulcra F and H to be immoveable, and the space under each piston to be a vacuum. Then E and C will move through equal spaces in equal times, and *m* will acquire just twice the velocity of *n*.

Here the force of P is twice that of Q, but the effects of these forces, if estimated by the product of each mass into its velocity, are equal.

3. In treating of rotatory motion;—in finding, for example, the centre of gyration of a mass revolving about a fixed point, the *rotatory force* of each particle is universally understood to be as the square of its distance from that point, or as the square of its velocity. If a body, A, (fig. 3.) be made to revolve about the centre C, by a force acting at P; four times that force, applied at the same point, P, will be required to make a body, B, equal to A, placed at twice the distance of A from C, revolve with the same angular velocity, that is, with twice the absolute velocity of A. If both the bodies be disengaged from C, they will each continue to move with the same velocity as before, but in rectilinear directions; and then the force of B is said to be only twice that of A. But it is not alledged that A can gain, or B lose force, by the mere circumstance of being disengaged from C. How then is this change in their relative forces to be accounted for?

4. Let the lengths of the arms A F, F B, (fig. 4.) of the balance beam, A B, be in the proportion of 1 to 2, and let the weight of the ball, m, be to that of n, as 2 to 1. If they

vibrate about the fixed fulcrum F, the quantity of motion of *m*, will be equal to the quantity of motion of *n*. Let C D be another balance beam, and let C G and G D be each equal to A F, and the weights of *o* and *p* be each equal to that of *m*, and let A and C move with equal velocities. If the quantity of motion of *m* be equal to that of *n*, the quantity of motion of *p* must also be equal to that of *n*; and the sum of the quantities of motion of *o* and *p* must be equal to the sum of the quantities of motion of *m* and *n*. But let both beams be at rest, and let the pressure of 2 be applied for a given time to C, to generate velocity in *o* and *p*; a pressure of 3 will be required to be applied to A for an equal time, and through an equal space, to generate an equal velocity in *m*. The generating forces, therefore, are as 2 to 3, although the quantities of motion generated by these forces are equal.

5. Let G (fig. 5.) be the centre of gravity of two bodies, A and B, connected by an elastic rod, at rest, but free to move in any direction; and let a given quantity of motion be communicated at any point, D, in a direction at right angles to the rod, Mr. Vince has demonstrated that the velocity of G will be the same wherever the motion is communi-

cated;* that is, if a given *force* be applied, or *quantity* of *motion* communicated at G, a progressive motion of the mass, without any rotatory motion, will be the result; but if the same force be applied at any other point D, we shall have the same progressive motion, and a rotatory motion besides.

Is that rotatory motion produced without force?

Examples of Motion destroyed, and of Motion transferred from one Body to another.

6. If the weight of the ball, A, (fig. 6.) be to that of B, as 2 to 1, and if they move in opposite directions with velocities reciprocally as their weights, and strike at the same instant the ends of the spring, S. If the strength of the spring be such, that the balls shall be at rest when its ends are brought to meet; they will meet at E, DE being equal to 2CE. Here the effect produced is the compression of the spring. But though the quantity of motion of A is equal to that of B, the portion of the effect produced by A, is less than that which is produced by B.

If we substitute for B a ball equal in weight

* Philosophical Transactions, vol. 70. p. 551.

and velocity to A, the ends of the spring will not be brought to meet by the action of the balls. In that case, when the balls are at rest, the distance between the ends of the spring will be to C D, as 1.1 to 6 nearly.

7. If a non-elastic mass, A, (fig. 7.) moving with a given velocity, strike an equal non-elastic mass, B, at rest in free space; both balls will move on together, with half the velocity of A. Upon the principle of the moving forces being as the quantities of motion, and the quantities of motion as the masses into their velocities; it is held that the moving force of A is equal to that of A and B, moving together with half the original velocity of A.

If the ball B, have a spring attached to it, furnished with a toothed catch C, to retain the spring in the form to which it may be compressed; it will then represent a perfectly non-elastic body. Let A strike the spring and compress it to E, and let A and B move on together, with half the original velocity of A. Let the spring be then removed in its compressed state, and placed between two other balls, C and D, equal in their masses to A and B, and at rest in free space; let the catch C, be then disengaged; the spring will resume its original shape, and the balls, C and D, will each move off with half the original velocity

of A; and we shall then have three masses besides A, each equal to A, moving with half the original velocity of A, and all of them deriving their motion from the original force of A.

8. Let A (fig. 8.) be a non-elastic soft mass, uniformly penetrable by the cylinder c; that is, the tenacity of the parts of A shall be such, that c shall meet with the same resistance at every point of its progress. Let A move with the velocity v, in the direction A B, against an immoveable obstacle, and be brought to rest by forcing the length E F of the cylinder into the ball. That penetration of c is, in this instance, the whole effect produced by the force of the motion of A. Let the operation be repeated, but instead of an immoveable obstacle, let B be a mass equal to A, in free space, but not penetrable by c: then the cylinder will be forced into A a depth equal only to $\frac{1}{2}$E F, and when the side of A has arrived opposite to H, the side of B will have arrived opposite to I, (as represented at No. 2.) and the velocity of both balls will be $\frac{1}{2}v$.

If we repeat the experiment with a ball of half the weight, and twice the velocity of A, striking B in free space, the effects will be very different. We must then have a longer cylin-

der; for the length of it forced into the ball will be $=\frac{4}{3}$ E F, and the velocity of both balls after collision will be $\frac{2}{3}v$. It is not easy to understand how these last effects can be produced by a force no greater than the first.

9. It is argued that the mass into the velocity must be the proper measure of the force of a body in motion, because the sum of the products of the various masses of any system of bodies into their respective velocities, is always the same in the same direction, unless acted upon by some external force. In other words, because the motion of the centre of gravity of any system of bodies cannot be changed or disturbed by any action of those bodies upon each other.

If two equal non-elastic balls A and B, whose common centre of gravity is G, (fig. 9.) move with the velocities and in the directions A C and B C, oblique to each other, they will meet at C, and after collision they will move on together with the velocity and in the direction G C. If the product of the mass into the velocity *in the same direction* be taken as the measure of the moving force, we have in the *motion* of these bodies, equal effects of force before and after collision. But it is obvious, that to produce the separate motions of A and B before collision, much

greater force must be required than to produce the motion of their joint mass.

10. If two elastic equal balls E and F, (fig. 10.) moving with the respective velocities A C and A B, at right angles to each other, strike at the same instant a third elastic ball A, equal to E or F; E and F will be brought to rest, and A will move off with the velocity and in the direction A D. In this case, the whole amount of the forces of E and F must have been communicated to A; but the velocity acquired by A is less than the sum of the velocities of E and F.

11. If the directions of E and F be not at right angles, (as in fig. 11.) the result will be as follows: produce A B, and draw the perpendicular D G. After the stroke, the velocity of A in the direction A B, will be $\frac{2AB \times AD}{AB + AG}$, and E and F will each continue to move in their first directions with the velocity $\frac{AB \times BG}{AB + AG}$.*

In this case, as in all others, the velocity and the direction of the centre of gravity of the system is, no doubt, the same before and after

* If BAC be an obtuse angle, the same solution applies, only E and F rebound instead of proceeding forward.

collision. But that is only one feature of the case. If we examine all the results after collision, we shall find that the motion of A is not the same as it would have been if it had been struck by a mass equal to E+F, having the same velocity as the common centre of gravity of E and F before collision. If, however, we reckon the forces as the masses into the squares of their absolute velocities, we shall (if they be perfectly elastic) always find that whatever force is lost by the striking balls, is gained by that which is struck.

12. Let four equal balls A, B, D, E, (fig. 12.) revolve about their common centre of gravity, C. Let A and B be connected by a rod of no sensible inertia, and D and E by a similar rod, but unconnected with A and B. Let the distance of the centres of gyration of A and B be twice that of D and E, and let D and E make two revolutions while A and B make one. If the balls and rods be elastic, and the velocity of each ball 10, and if the rod connecting A and B be struck by the balls D and E at their centre of gyration, the velocity of A and B after the stroke will be 14, and that of D and E will be 2. If the balls and rods be non-elastic, the velocity of A and B after the stroke will be 12, and that of D and E, 6.

In the first case, the sum of the products of the masses into the squares of their respective velocities, is the same before and after collision; but in the second case, that sum is less after than before collision; and it must, I presume, be admitted, that the rotatory force in that case is diminished by the collision.

13. If an iron prism A B, (fig. 13.) moveable on a fixt centre at A, be let fall on a piece of soft clay C, the greatest impression might be expected to be made when the clay is placed under P, the centre of percussion of the prism. But if the experiment be made, the impression will be found to be the same, whether the clay be placed at C, D, or E, or at any other distance from the centre of motion.

14. Let two equal elastic balls A and B, (fig. 14.) be connected by an elastic rod, and be at rest in free space, and let G be their common centre of gravity. If another elastic ball C, whose mass is equal to the joint masses of A, B, and the rod, moving with the velocity v in the direction C G at right angles to the rod, strike it at G; C will be brought to rest, and G will move off with the velocity v, in the direction C G. But if we repeat the experiment, applying the force of D instead of C, the mass of D being equal to that of A or B and half the rod; and its velocity equal $2v$, striking

A at its centre of gyration around the point, G, the result will be as follows: D will be brought to rest, G will move off as before with the velocity v, and A and B will have a rotatory motion about G, with the velocity v at their centres of gyration. In both instances, the striking forces, if measured by the masses into their velocities are the same; and as the striking balls are in both instances brought to rest, they must have communicated exactly their whole force to the mass which was struck. The results, however, are far from being equal. If the force of D be no greater than that of C, we shall have the rotatory motion produced without force, although we have no reason to suppose that the rotatory can be produced with less force than the rectilinear motion.

In order to avoid unnecessary calculations or analyses, I have stated these cases in the most simple forms I could devise. I am aware that there are many who think they may be easily solved in the usual way, and that some of the cases will be considered as trivial paradoxes. But if we examine the explanations which have been given of similar cases, we shall find that there is considerable diversity of opinion about the principles by which they are to be explained; and that some of the solutions

are not quite so obvious as, at first sight, they appear to be.

Before we enter upon the examination of these particular cases, it may be proper to observe, in addition to what has been already noticed; that, in respect to the general question, or in respect to the existence even of any question at all on this subject, some of the best recent authorities are the most difficult to be reconciled with each other.

Few authors, in our language, on the principles of mechanics, have been more generally read and referred to than Emerson. From the great analytical skill of this author, one would have expected something decisive on the long pending question concerning the measure of moving force; but he seems to take for granted, that the measure is the mass into the velocity or the *momentum*, for he scarcely condescends to mention the other, and after a few observations, dismisses it in the following laconic manner:—"It seems to be a necessary property of the *vis viva*, that the resistance is uniform. But there are infinite cases where this does not happen; and in such cases, this law of the *vis viva* must fail. And since it fails in so many cases, and is so obscure in itself, it ought to be weeded out, and not to pass for a principle in mechanics."*

* Emerson's Principles of Mechanics, p. 20.

Mr. Atwood, however, has shewn that Mr. Emerson himself has been led into error, by neglecting this very principle which he proposes to weed out. In reference to a particular problem, he says, "In Emerson's Fluxions, p. 177,* there is this problem: The radii of a wheel and axle are given in the proportion of $b : a$; a weight w acting by means of a line on the circumference of the wheel, elevates a weight y suspended from a line which goes round the axle; it is required to assign the quantity y, when $y \times$ into its velocity generated in a given time, is the greatest possible."

"In the solution, the author supposes the momentum of bodies to be as the quantity of matter into the velocity generated; and according to the usual doctrine of momentum, assumes it as an universal truth, that if a force acts on any different quantities of matter for a given time, it will always generate the same moment, estimated by the quantity of matter into the velocity. From this reasoning he deduces the weight sought, $y = \sqrt{2} - 1 \times \frac{bw}{a}$ when its true value is $y = w \times \sqrt{\frac{b^4}{a^4} + \frac{b^3}{a^3} - \frac{b^2}{a^2}}$ (page 249.) agreeing with the former only in the extreme case when $b = a$, that is, when the radius of the wheel is equal to that of the axle."†

* 2d Edit. † On Rectilineal Motion, Preface, p. x.

Mr. Smeaton, at the commencement of the description of his experiments on water-wheels, says—"The word *power*, as used in practical mechanics, I apprehend to signify the exertion of strength, gravitation, impulse, or pressure, so as to produce motion."* And near the end of his "*Experimental Examination*," we have the following conclusion:—

"It therefore directly follows, conformably to what has been deduced from the experiments, that the mechanic power that must of necessity be employed in giving different degrees of velocity to the same body, must be as the square of that velocity." And in the next page he observes, "It seems, therefore, that without taking in the collateral circumstances both of time and space, the terms quantity of motion, *momentum*, and force of bodies in motion, are absolutely indefinite; and that they cannot be so easily, distinctly, and fundamentally compared, as by having recourse to the common measure, viz. mechanic power."†

M. De Prony, however, gives a different conclusion, as follows: "Il y a eu des disputes très vives parmi les mathématiciens pour savoir si on devoit faire la *force* d'un corps en mouvement proportionelle à la vîtesse ou au quarré

* Philos. Trans. 1795, p. 105. † Ibid, 1776, p. 473.

de la vitesse: il est bien aisé, d'après tout ce qui précede, de réduire la question à un énoncé raisonnable qui en suggérera sur-le-champ la solution. Le mot *force* ne désignant qu'une cause dont la nature est inconnue, et dont les effets sont les seules choses que nous puissions mesurer, il est clair qui par ce mot *mesure de la force*, on ne peut entendre que celle d'un de ses effets; or ces effets pouvant se considérer sous différents aspects, chacun comporte une espece de mesure particuliere et conforme à sa nature. Cela posé, si l'on considere l'effet de la force comme consistant dans la destruction d'une certaine somme d'obstacles ou de quantités de mouvement, cette somme est proportionnelle à la simple vitesse. Si on ne considere point l'effet de la force relativement à la somme des obstacles vaincus, mais relativement à leur nombre, ce nombre sera proportionnelle au quarré de la vitesse lorsque tous les obstacles seront égaux.*

* What is here meant by the *sum* and the *number* of obstacles, is not very obvious. That explanation has, however, been adopted by various other authors. It appears to have originated with D'Alembert, who states it thus: "Donc dans l'équilibre le produit de la masse par la vitesse, ou ce qui est la même chose, la quantité de mouvement, peut représenter la force. Tout le monde convient aussi que dans le mouvement retardé, le nombre des obstacles vaincus est comme le quarré de la vitesse; ensorte qu'un corps qui a

"On voit par-là que la fameuse question des *forces vives* n'est qu'une dispute de mots qui n'auroit jamais subsisté si l'on avoit voulu s'entendre, c'est a dire analyser et definir."†

fermé un ressort, par example, avec une certaine vitesse, pourra avec une vitesse double fermer, ou tout à la fois, ou successivement, non pas deux, mais quatre ressorts semblables au premiere, neuf avec une vitesse triple, & ainsi du reste."——"Il faut avouer cependant, que l'opinion de ceux qui regardent la force comme le produit de la masse par la vitesse, peut avoir lieu non-seulement dans le cas de l'équilibre; mais aussi dans celui du mouvement retardé, si dans ce dernier cas on mesure la force, non par la quantité absolue des obstacles, mais par la somme des resistances de ces mêmes obstacles. Car on ne sauroit douter que cette somme de résistances ne soit proportionnelle à la quantité de mouvement, puisque, de l'aveu de tout le monde, la quantité de mouvement qui le corps perd à chaque instant, est proportionnelle au produit de la resistance par la durée infinement petite de l'instant, & que la somme de ces produits est évidemment la résistance totale. Toute la difficulté se reduit donc à savoir si on doit mesurer la force par la quantité absolue des obstacles, ou par la somme de leurs résistances. Il paroitroit plus naturel de mesurer la force de cette dernier maniere, &c."* But it should be remarked, that although equal quantities of motion are lost in equal times, it is not universally acknowledged that these equal times denote equal quantities of force, or equal quantities of resistance. That indeed is the very question at issue.

* Traité de Dynamique Discours Prelim. p. 20 et 21.

† Arch. Hydr. p. 24.

On the other hand, Dr. Milner, of Cambridge, holds, "that it is plain, that if any one contends for the equality of action and reaction, and explains those terms by the change produced in the absolute forces of bodies, the dispute is not merely verbal."* And again, he says, "some writers have considered this question as entirely verbal, and have affected to treat the advocates on both sides with the greatest contempt. Such persons save themselves a great deal of trouble, and have the credit of seeing farther into the controversy than others; but after all, I am afraid the practical mechanic will receive little information or security from such speculations."+

Dr. Wollaston's opinion is, that "the conception of a quantity dependent on the continuance of a given *vis motrix* for a certain *time* may have its use, when correctly applied, in certain philosophical considerations; but the idea of a quantity resulting from the same force exerted through a determinate *space* is of greater practical utility, as it occurs daily in the usual occupations of men."‡ And he concludes his lecture on the Force of Percussion thus: "In short, whether we

* Philosophical Trans. 1775, p. 377. + Ibid p. 378.

‡ Philos. Trans. 1805, p. 1

are considering the sources of extended exertion or of accumulated energy, whether we compare the accumulated forces themselves by their gradual or by their sudden effects, the idea of mechanic force in practice is always the same, and is proportional to the *space* through which any moving force is exerted or overcome, or to the *square* of the velocity of a body in which such force is accumulated." This conclusion coincides nearly with Mr. Smeaton's, but still it remains to be explained how two given quantities of force may, consistently, be considered as equal to each other for philosophical purposes, but unequal for all practical purposes.

The Edinburgh reviewers of Dr. Wollaston's lecture, adopt a different doctrine. In reference to the first passage quoted above, they say, "Now, with the judgment here given as to the respective utility of the two measures of the force of moving bodies, we cannot entirely agree; though we differ from Dr. Wollaston with considerable diffidence; and the more, that his opinion is supported by one of the greatest authorities in practical mechanics of which this or any other country can boast—the late Mr. Smeaton."* And after some

* Edinb. Review, vol. 12, p. 122.

remarks on supposed errors of Mr. Smeaton, which I shall have occasion to refer to again, they say, "To whatever cause, therefore, the imperfection of the theory of the machines moved by water is to be ascribed, it is not to any thing that would be corrected by the introduction of a measure of force different from that which is commonly in use."* At the beginning, however, of the same article, they give the following opinion: "It is no longer doubted that this force (of percussion) may, with perfect truth, be considered as proportional, either to the quantity of matter multiplied into the velocity, or to the quantity of matter multiplied into the square of the velocity, according to the nature of the effect which it is destined to produce."†

On the subject of forces, M. Laplace expresses himself as follows: "La force peut etre exprimée par une infinité de fonctions de la vitesse, qui n'impliquent point contradiction. Il n'y en a point, par exemple, a la supposer proportionnelle au carré de la vitesse."‡ After stating a hypothetical example of force, where the results would be different from those of experience, but where the square of the velo-

* Edin. Rev. vol. 12, p. 126. † Ibid. p. 120.

‡ Système du Monde, 3d edit. Liv. III. ch. 2. p. 141.

city is taken in a sense quite different from that in which it appears to have been understood by every other author I have had an opportunity of consulting, he proceeds:—"Parmi toutes les fonctions mathématiquement possibles, examinons quelle est celle de la nature." And after reasoning at some length on various effects of force he concludes, "Voila donc deux lois du mouvement, savoir, la lois d'inertie et celle de la force proportionnelle á la vitesse, qui sont données par l'observation. Elles sont les plus naturelles et les plus simples que l'on puisse imaginer, et sans doute, elles dérivent de la nature meme de la matière; mais cette nature étant inconnue, ces lois ne sont pour nous, que des faits observés, les seuls, au reste, que la mécanique emprounte de l'expérience."*

It appears then to be the opinion of this distinguished philosopher, that, although it may be mathematically possible for the force of a body in motion to be proportional to the square of its velocity, yet such a principle is inconsistent with the phenomena of nature; but that the law of inertia, and the law of force proportional to the velocity, are the most natutural and the most simple principles imaginable, that they are derived from the very

* Système du Monde, p. 144.

nature of matter, and that they are the only facts which the science of mechanics borrows from experience.

It may be proper to observe here, that M. Laplace adopts as first principles, only the two first of Sir Isaac Newton's laws of motion.

It is surprising that so many different opinions on this subject should still be held, and it is not easy to understand how so many good reasoners have, from the same data, drawn conclusions so much at variance with each other.

Fifty years ago, M. D'Alembert, speaking of the science of mechanics, observed, that "En général, on a été plus occupé jusqu'à présent a augmenter l'edifice qu'a en éclairer l'entree; et on a pense principalement à l'élever, sans donner a ses fondemens toute la solidité convenable."*

No one will deny, that, during the last fifty years, great advances have been made in the application of mechanical principles to the investigation of the motions of the heavenly bodies. But as far as these principles have been adapted to practical uses, may not M. D'Alembert's observation be with some justice applied to the present state of mechanical science? or may it not be said, that, not only

* Traite de Dynamique, Discours prelim. p. 4.

the entrance, but the interior of the structure is not very conveniently arranged for the occupations of life?

But there is another observation of M. D'Alembert, which has, on the present occasion, still stronger claims on *my* attention. He says, "mais il semble que la plûpart de ceux qui ont traité la question de la mesure des forces, ayent craint de la traiter en peu de mots."

Although the censure be severe, it may be just, and I shall endeavour to profit by it. Some repetitions, however, in discussions of this kind, are unavoidable.

In the observations which I have made, as well as in those which I have still to make on various passages in some of the best authors on mechanics, I hope to escape the charge of being in any degree disrespectful towards them. I am sensible that any remarks having that tendency would ill become me, and could be of no avail in my argument. Anxious as I am to state distinctly the reasoning and the conclusions which appear to me to be objectionable, I am not less anxious to state them fairly and respectfully. I am well aware of the disadvantages under which I labour; the general prejudice against this subject being so strong, that a great national institution has

absolutely proscribed the discussion of it.* That circumstance, however, enhances the value of the indulgence, of which I now avail myself, in submitting it to the consideration of this society.

Proceeding now to the consideration of the particular cases which I have described, I may observe, that the first two cases (p. 115.) comprehend, I believe, the chief points at issue, as far as they relate to force producing rectilinear motion by the intervention of levers or wheels, and to motion produced about fixed axes.

That the forces of C and D in the first case are equal, cannot, I think, be questioned; and it is not less obvious that their effects, if estimated by the masses into the squares of their velocities, are also equal.

In the second case, the force of P is twice that of Q, and the effects of these forces, if measured by the masses into the squares of their velocities, are respectively in the same proportion.

Mr. Atwood (as we have already noticed at page 109) admits, that the measure composed of the mass into the square of its velocity

* The French National Institute has, I understand, prohibited the reception of all dissertations on the measure of force.

obtains in all cases of rotatory motion about fixed axes; and that the measure composed of the mass into its velocity, when applied to the same cases, fails; "a given quantity of motion thus estimated, being alterable in any assigned ratio."

But authors on mechanics generally concur in the following conclusion: that "a distinction is always to be made between the actions of bodies when at liberty, and when they revolve about a centre or axis. In the first case, the motion lost is always equal to the motion communicated in an opposite direction: in the second, the motion lost is to be encreased or diminished in the ratio of the levers before it will be equal to the motion communicated."*

We do not find, however, that the absolute forces, or their effects, can be encreased or diminished by any alteration in the lengths of the levers. For if the arm H G, for example, be extended to any assumed length, the same velocity will still be produced in *m* by the motion of P through the same space. It is true the velocity will not be produced in the same time; but the result will be the same, in whatever time, or by whatever complication of levers or wheels, it may be produced.

* Dr. Milner. Philos. Trans. 1776, p. 371.

The converse of this case is stated by Dr. Wollaston, as follows: "It may be of use also to consider another application of the same energy, and to shew more generally that the same quantity of total effect would be the consequence not only of direct action of bodies upon each other, but also of their indirect action through the medium of any mechanical advantage or disadvantage; although the time of action might by that means be encreased or decreased in any desired proportion. For instance, if the body supposed to be in motion were to act by means of a lever upon a spring placed at a certain distance from the centre of motion, the retarding force opposed to it would be inversely as the distance of the body from the centre; and since the space through which the body would move to lose its whole velocity would be reciprocally as the retarding force, the angular motion of the lever and space through which the spring must bend, would be the same, at whatever point of the lever the body acted."* Practical men are much beholden to Dr. Wollaston. He is, I believe, the only author, professedly on the theoretical principles of mechanics, who has written decidedly in support of Mr. Smeaton's

* Philos. Trans. 1806, p. 21.

conclusions, and we have only to regret that he has not pursued the subject farther.

If the amount of the force could be encreased or diminished by any variation of the length of the lever, we might expect to find its measure to be of that indefinite kind which might be estimated by the product of the mass into *any function* of its *velocity*. Such a conclusion, however, is quite inconsistent with experience; for under every variation of the proportions of the lever, the effect, if measured by the mass into the square of its velocity, is uniformly found to be in proportion to the moving force by which it is produced; if that force be measured by the pressure multiplied into the space through which it acts. But if we multiply the mass into any other function than the square of its velocity, no such general correspondence between the force and its effects is to be found.

Mr. Smeaton has well illustrated this principle by many valuable experiments on the more complicated cases of the action of water on mill-wheels, and on force generating rotatory motion in masses of matter about fixed axes.*

The Edinburgh reviewers of Dr. Wollaston's lecture on the force of percussion, have urged

* See Philos. Trans. for 1759 and 1776.

some strong objections against Mr. Smeaton's conclusions. I would willingly excuse myself from venturing to controvert any thing in a criticism written with so much candour and ability; but some of the arguments it contains are pressed so powerfully against the application of the square of the velocity of a body in motion as the measure of its force, that they must, I apprehend, be answered before that measure can be consistently defended.

In the first place, it is argued, that the principle which Mr. Smeaton understood to be confirmed by the result of all his experiments, "is in fact abandoned by him at the very outset of his investigation, in consequence of having included the time in the measure of the effect."* Now, I do not see how this supposed contradiction in Mr. Smeaton's reasoning can possibly be maintained. The measure of mechanical power adopted by him, consists of the pressure multiplied into the space through which it acts. In cases where the pressure moves through equal spaces in equal times, it can make no difference whether the *time* or the *space* be taken as an element of the mechanical power; and when, in such cases, Mr. Smeaton takes either of these, it does not follow that he abandons the other.

* Edinb. Review, vol. 12, p. 123.

He does not say that the consideration of the time is necessarily excluded, he only says it is not necessarily included in the estimation of mechanical power; and he has (at the conclusion of the passage referred to by the reviewers) taken care to discriminate the particular cases in which the time may or may not be so taken into consideration. He says, "but *note* all this, (relating to the quantity of power expended in raising a known weight with a uniform velocity to a known height) is to be understood in the case of slow or equable motions of the body raised; for in quick, accelerated, or retarded motion, the *vis inertiæ*, of the body moved will make a variation."*

He might indeed, consistently with his principles, have excluded altogether the consideration of the time in which any mechanical effect is produced. For, according to these principles, the same quantity of mechanical power is required to raise a given weight to a given height, in whatever time it may be effected, or whether the motion be equable or not, *provided that the velocity of the weight at the beginning and the end of the operation be the same.*† Accordingly he says, "from

* Philosophical Trans. 1759, p. 106.

† It is, I presume, hardly necessary to say, that when the motion of the weight is so quick as to make the resistance of

of the effect produced. The current of the water being uniform, he first ascertains (by means of a pump which supplies it) the quantity which *passes in one minute*, and then he makes various experiments to ascertain the greatest effect that can be produced by that quantity, by merely multiplying, after every experiment, the weight into the height to which it is raised in a minute. Now the time of *one minute* is taken merely because it is known that a certain quantity of water passes in that time—the effect which is to be estimated, being produced in the same time. But the time is by no means a necessary element in the estimation of the effect; for the height to which a weight is raised by any other given quantity of the running water, may easily be determined without reference to the time, and the result will be the same as when the time is considered. Let p, for example, represent the power, that is, a given quantity of water moving with a given velocity, and e the effect, or the product of the weight into the height to which it is raised by that power, without any reference to the time in which it is raised. Let p' be any other quantity of water moving (for the sake of simplicity) with the same velocity, and e' its effect. Now, if the power be equally well applied in both cases,

and if we have adopted a proper measure in estimating the effect, we shall have $\frac{p}{c}=\frac{p'}{c'}$. It is obvious that this equation will constantly be found by Mr. Smeaton's method, and we must therefore conclude that he has adopted the proper measure of the force.

But Mr. Smeaton's reasoning is farther objected to as follows: "His second general maxim is, that the expence of water being the same, the effect will be nearly as the height of the effective head, or (as it is expressed in maxim third) as the square of the velocity of the water. This conclusion seems, at first sight, quite in favour of the theory of mechanical force, as laid down by our author, and the other supporters of the *vis viva;* and yet we shall presently find, that it is perfectly conformable to the other theory, and to those reasonings of Desaguliers and Maclaurin, which Mr. Smeaton has censured, as leading to conclusions altogether wide of the truth."

"Let c be the velocity of the stream, v that of the wheel, A the area of the part of the float-board immersed in the water, g the velocity which a heavy body acquires in one second when falling freely. Then $c-v$ will be the relative velocity of the stream and the wheel, or the velocity with which the water strikes the wheel; and if we take h, a fourth proportional

to g^2, $(c-v)^2$ and $\frac{1}{2}g$, h will be the height from which a body must fall to acquire the velocity $c-v$, and will be $=\frac{(c-v)^2}{2g}$. Wherefore, by a proposition, well known in Hydraulics, the circumference of the wheel is urged by the weight of a column of water, of which the section is A, and the height $\frac{(c-v)^2}{2g}$, and of which the solidity is therefore $A\times\frac{(c-v)^2}{2g}$. Thus far the investigation is applicable to all undershot wheels, and to all hydraulic engines of a similar construction."*

Now, before we proceed to the remainder of this demonstration,† which is grounded upon the supposed certainty of this last conclusion, let us see how far this theory agrees with the results of Mr. Smeaton's experiments.

Let w represent the weight of the column, the solidity of which is expressed by $A\times\frac{(c-v)^2}{2g}$. The value of w in Mr. Smeaton's experiments is easily found; and he has furnished data by which we can determine nearly the pressure by which the circumference of the wheel is urged. Let p represent that pressure; then, if the experiments agree with the theory, we

* Edinb. Review, vol. 12, p. 124.

† Namely, that the maximum effect must be produced when $v=\frac{1}{3}c$, and that it is proportional to c^2.

should always have $p=w$. But we shall look in vain to the results of Mr. Smeaton's experiments for this equation. I subjoin the comparative values of p and w, calculated from Mr. Smeaton's first table of eight experiments:*

EXPER. 1. $p = 2.3w$
2. $p = 2.37w$
3. $p = 2.15w$
4. $p = 2.22w$
5. $p = 2.16w$
6. $p = 2.11w$
7. $p = 2.01w$
8. $p = 1.85w$

And in the 27th Ex. p. 115, we have $p = 2.7w$.

If these results be correctly stated, Mr. Smeaton might truly say, that he "found these matters to come out in the experiments, very

* If Mr. Smeaton's reduction of his 5th Experiment, page 112, be compared with the table page 110, it will appear, that he has omitted to include in the quantities set down in the table, the weight of the scale, pulley, and counter-weight. In finding the value of p, I have, in each experiment, taken twice the weight of the scale and pulley, added to the counter-weight, to be equal to 1.37 lb. which will be near enough for the purpose of comparison.

It should be observed also, that if the table had been made out in the same way, the fourth experiment would have given the maximum effect.

different from the opinions and calculations of authors of the first reputation."*

It is true, Mr. Smeaton's maxims agree with some of the results brought out by the common theory. His maxims, however, are by no means the most important conclusions which he has drawn from the results of his experiments; neither can I agree with the reviewers in supposing that he considered these maxims to be inconsistent with the common theory. If it were admitted, according to the theory, that the pressure at the circumference of the wheel is always as $A \times (c - v)^2$ we can hardly suppose Mr. Smeaton to have been so little acquainted with the principles of calculation as not to have been aware that the maximum effect must consequently be as $A \times c^2$. The principle of the *vis viva* agrees still more remarkably with the common theory in cases of rotatory motion generated about fixed axes, as I have already observed at page 117. But, although the rotatory force of a body in motion is, according to the common theory, as the square of its velocity, I do not see why that agreement with the principle of the *vis viva* should be brought as an objection against it. The chief object in discussion is to ascertain

* Philo. Trans. 1776, p 457.

upon which principle the most consistent explanation of the facts is to be obtained in cases where the two measures disagree.

It appears to me that Mr. Smeaton's four maxims on undershot water-wheels may all be comprehended in one, expressed thus: *That in cases where the maximum effect is produced, it is nearly as the quantity of water multiplied by the effective head.** But the theory is founded on the supposition that *in all cases* the pressure at the circumference of the wheel is as $(c-v)^2$, and if it were so, the maximum effect would, no doubt, be produced when $v=\frac{1}{3}c$. By the mere inspection, however, of the results which I have stated above, it will be seen that the pressure at the circumference of the wheel is not as $(c-v)^2$ and therefore, the maximum effect cannot be produced when the wheel moves with one-third of the velocity of the water.

I have to regret that I cannot at present refer to M. Bossut's experiments on water-wheels. It is observed, however, by M. du Buat, that according to these experiments, the maximum effect was produced when the velocity of the wheel was $\frac{4}{9}$ that of the water, which corre-

* It should be observed, that the maximum effect was not always produced at the same relative velocity.

sponds very nearly with Mr. Smeaton's conclusions.

From that result, M. du Buat concludes that the pressure at the circumference of the wheel is as $(c-v)^{\frac{1}{4}}$*. After highly commending the experiments and observations of M. Bossut, M. du Buat continues: "Nous avouons néanmoins, à regret, que, quelque nombreuses et variées qu'elles soient, elles ne sont pas encore suffisantes pour être applicables à tous les cas. Ce ne sera qu'après en avoir fait de nouvelles sur le même plan, et en avoir rapporté les résultats à quelque loi d'approximation simple, telle que celle que nous avons exposée, qu'on pourra espérer de donner des règles pratiques propres à guider les artisans auxquels ces sortes de constructions sont abandonées."† This observation well merits the attention of every writer on theories of hydraulics. Whether we contemplate the number and diversity of the theories which have been proposed, or the still greater number of facts which appear to be beyond the reach of mathematical explanations, it must, I apprehend, be obvious, that approximation by experiment is all that can, in the present state of the science, be reasonably expected in the comparison or estimation of hydraulic forces; and we have a

* Principes d'hydraul. vol. 2. p. 356. † Ibid, p. 360.

convincing proof of the great caution with which such approximations should be sought, in the mistake into which this ingenious, persevering, and skilful experimenter has himself been led, by attempting to generalize too far the results of some of his experiments—I allude to his peculiar theory of *non-pressures*. After very reasonably concluding, that, in cases where water is descending, as it were upon an inclined plane, the bottom of the channel does not sustain the whole weight of the water, he extends that principle as follows: "Si, par une cause quelconque, une colonne fluide comprise dans un fluide indéfini, ou contenue dans des parois solides, vient à se mouvoir avec une vitesse donnée, la pression qu'elle exerçoit latéralement avant son mouvement contre le fluide ambiant, ou contre la paroi solide, sera diminuée de toute celle qui est dûe à la vitesse avec laquelle elle se meut."* Now this doctrine is obviously untenable. For, when water is moving upon a horizontal plane, we cannot doubt that the plane must support the whole weight of the water. It is never supposed that a ball loses a part of its weight by rolling upon a horizontal plane, excepting indeed the amount of

* Principes d'hydraul. vol. 2, p. 175.

its centrifugal force from the centre of the earth; but that exception does not apply to the case in question, for the centrifugal force, whatever it is, must, according to M. du Buat's theory, be added to the non-pressure. In confirmation of his theory of non-pressures, M. du Buat observes, "Qu'ayant fait mouvoir, à une certaine profondeur, dans une eau stagnante, un tube vertical ouvert par les deux bouts, dont le supérieur étoit hors de l'eau, le fluide s'est maintenue dans le tube, plus bas que la superficie du réservoir, d'une quantité à-peu-près égale à la hauteur dûe à la vitesse avec laquelle il étoit mu."* But he has omitted to take into consideration the cohesion or the lateral action of the particles of the water upon each other, which has since been so well observed by M. Venturi; from whose experiments, and from those of Dr. Matthew Young,† made under the receiver of an air-pump, we may safely conclude, that, were it not for the pressure of the atmosphere, and the cohesion of the particles, there could be no depression in the tube as observed by M. du Buat; and, had he been aware of these circumstances, he surely would never have reasoned as he has done on the subject of

* Principes d'hydraul. Vol. 2. p. 156.

† Irish Philos. Trans. vol. 7. p. 63.

non-pressures. But to return to the subject of water-wheels.

It has been attempted to be theoretically demonstrated by M. de Borda, and afterwards by Mr. Waring, of America, that the force of the water against the wheel is not proportional to the square of the velocity with which it strikes the wheel, but that it is in the simple ratio of that velocity ; and that the maximum effect is therefore produced when the velocity of the wheel is half that of the stream.

M. de Borda, in reference to the labours of others, says, " On ne considéroit qu'une seule palette contre laquelle on cherchoit la force du choc du fluide;---mais il falloit observer que dans le mouvement dont il s'agit, l'action du l'eau ne s'exerce pas contre une palette isolée, mais contre plusieures palettes à la fois, et que ces palettes fermant tout le passage du petit canal et ôtant au fluide la vîtesse qu'il a de plus qu'elles, la quantité du mouvement perdu par ce fluide, et par consequent le choc qu'éprouvent les palettes, n'est plus proportionnelle au carré de la difference des vîtesses du fluide et des palettes, mais seulement à la difference de ces vîtesses."*

* Memoires de l'Acad. Paris, 1767, p. 274.

Mr. Waring's demonstration is as follows: "If the relative velocity of a fluid against a single plane be varied, either by the motion of the plane, or of the fluid from a given aperture, or both, then, the number of particles acting on the plane in a given time, and likewise the momentum of each particle, being respectively as the relative velocity, the force on both these accounts, must be in the *duplicate* ratio of the relative velocity, agreeably to the common theory, with respect to this *single plane*; but, the number of these planes, or parts of the wheel acted on in a given time, will be as the velocity of the wheel, or *inversely as the relative velocity*; therefore the moving force of the wheel must be in the simple direct ratio of the relative velocity," and, consequently, the maximum effect must be produced when the velocity of the wheel is half that of the water.*

But this kind of demonstration cannot, I think, be very satisfactory. It leads, I apprehend, to this conclusion, that we may double the power of any undershot water-wheel, (whatever may be its velocity) by merely doubling the number of its floats or planes acted upon by the water. Mr. Smeaton, how-

* American Philos. Trans. vol. 3. p. 146.

ever, found, that no such advantage was to be gained by that means.*

It must be acknowledged, that the celebrated experiments of D'Alembert, Condorcet, and Bossut, furnished results in confirmation of the common theory. But these were made under particular circumstances; they did not comprehend a sufficient variety of depths and velocities to afford satisfactory conclusions as to the general question, and various deductions, of rather an arbitrary kind, were made from the actual pressure before the result which agreed with the theory was brought out.

On the other hand, we have many experiments which are quite at variance with the theory. We may, in particular, refer to those of Don Juan and M. du Buat. The former exposed to a current of water, moving with the velocity of two English feet in a second, a plane of one square foot, immersed one foot under the surface, and found that it supported a weight of $15\frac{1}{2}$ lb. which is nearly four times the weight it should have supported, according to the theory.† M. du Buat exposed to a current, having the velocity of three French feet in a second, a plane of one square foot,

* Philos. Trans. 1759, p. 124.

† De Prony Arch. Hydr. p. 394.

immersed three inches under the surface, and found that it supported a weight of 19.45 liv. which, by the theory, should have been only 8.75 liv.* M. de Prony attempts to account for the results obtained by Don Juan, by the additional pressure occasioned by the surface of the water over the plane being raised higher than the general level of the current. That circumstance, however, can account for a small part only of the difference. M. du Buat explains his experiments by his theory of non-pressures, which I have already shown to be fallacious.

M. du Buat has described other experiments which are considered by some to accord better with the theory.† They were made upon insulated veins of water, spouting from the perpendicular side of a vessel against a surface not greater than the section of the vein; and from their results he draws the following conclusions: "Il résulte des expériences qui précèdent, que le choc d'une colonne ou d'une veine fluide contre une surface de même étendue & directe, est sensiblement égal au produit de cette surface, par la hauteur dûe à la vitesse. L'intensité du choc dépend néanmoins en partie de la liberté plus au moins

* Principes d'hydraul. vol. 2. p. 218. † Ibid, p. 142, &c.

grande que les filets ont de se dévier aux approches de cette surface; mais si la veine rencontre une surface plus grande qu'elle, qui l'oblige à changer en entier la direction de tous ses filets, la vitesse perdue, étant par là augmentée, la resistance devient beaucoup plus grande."*

But in these experiments, a part only of the vein strikes the surface opposed to it, and the force of that part appears to be equal to the force assigned by the theory to the whole vein.

Of all theoretical propositions, that which was first demonstrated by Daniel Bernoulli in his Hydrodynamics, page 290, and afterwards more fully by the same author, in the Comment. Petropol. vol. 8. page 120, appears to be the most applicable to Mr. Smeaton's cases, and comes the nearest to his results. It is, that, when the force of an insulated vein of water is directed perpendicularly against a plane indefinitely large, its pressure against the plane is equal to the weight of a column of water, of which the base is equal to the area of the section of the vein, and the height equal to twice the height due to the velocity of the vein. But the circumstances of this case are not quite the same as those of Mr. Smeaton, and he found the pressure against

* Principes d'hydraul. vol. 2. p. 150.

the plane to be still greater than the weight of a column of twice the height due to the relative velocity of the water and the wheel.

The most important conclusions drawn by Mr. Smeaton from his experiments are (as I have already noticed) not in his maxims; but they are to be found, I apprehend, in the two following observations, which I shall quote in his own words:

1. "It is somewhat remarkable," he says, "that though the velocity of the wheel in relation to the water turns out greater than $\frac{1}{2}$ of the velocity of the water, yet the impulse of the water, in the case of a *maximum*, is more than double of what is assigned by the theory.*

2. "We have seen before, in our observations upon the effects of undershot wheels, that the general ratio of the power to the effect, when greatest, was 3:1; *the effect, therefore, of overshot wheels, under the same circumstances of quantity and fall, is at a medium double that of undershot:* and as a consequence thereof, *that non-elastic bodies, when acting by their impulse or collision, communicate only a part of their original power;* the other part being spent in changing their figure in consequence of the stroke."†

* Philos. Trans. 1759, p. 113. † Ibid, 1759, p. 150.

It was chiefly in this last consideration that he found the prevailing theory to be defective; for, according to that theory, as it is applied in explaining the collision of bodies, there can be no force spent in producing change of figure: and it is very remarkable, that no succeeding writer has, as far as I can learn, paid any attention to this circumstance.

However much Mr. Smeaton's valuable observations may have been disregarded by authors, they have not been lost to practical men. Before the publication of the paper which I have been endeavouring to defend, several mills had been constructed under Mr. Smeaton's direction, in which his chief object was to apply the water so that less of its force should be expended in producing change of figure, and consequently more of its force be communicated to the wheel. Although he had obtained by his experiments results which were "more than double of what is assigned by the theory," yet by comparing the *effective* with the real head, he found that nearly half the power was, in many instances, spent in producing a change of figure in the water, before it reached the wheel; and still finding (as stated above in the second observation) that more than half of what remained of the

power was spent in the same way, by the manner in which it acted upon the wheel; he determined to apply the water, in all cases, so that it should act more by its weight, and less by its impulse; and the advantage gained by that improved construction was found to be fully equal to his expectations. It was afterwards so generally adopted and improved upon by himself and by other engineers in this country, that although undershot water-wheels were, about fifty years ago, the most prevalent, they are now rarely to be met with; and wherever the economy of power is an object, no new ones are made. So that all the points in question, as far as they relate to undershot water-wheels, although highly important at the time when Mr. Smeaton wrote his first paper, are now become matters of mere speculative curiosity, and, in this country at least, they can no longer be of any practical use. The question, however, respecting that part of the power which is expended in producing a change of figure, is highly interesting in other points of view, and we shall have occasion to consider it more fully when we come to examine the 6th, 7th, 8th, 9th, 12th, and 13th cases.

Dr. Milner, in allusion to Mr. Smeaton's remarks on the theory, observes that, "It is

acknowledged, that the experiments which have been made to determine the effects of wind and water-mills do not agree with the computations of mathematicians; but this is no objection to the principles here maintained. Writers generally propose such examples with a view rather of illustrating the methods of calculation by algebra and fluxions, than of making any useful improvements in practice. They suppose the particles of the water to move in straight lines, and to strike the machine with a certain velocity; and after that to have no more effect. As such suppositions are evidently inconsistent with the known properties of a fluid, we are not at a loss to account for a difference between experiment and theory; and therefore it should seem unreasonable to assert, that certain authors of reputation have neglected the collateral circumstances of time, space, or velocity in the resolution of these problems, unless we are able to point out such omissions."* But if the theory be applicable to speculative objects only, why are its conclusions laid down as rules to be adopted in practice? Mr. Smeaton objected to the practical application of the theory by the distinguished authors which he quoted, because they omitted to take into

* Philos. Trans. 1778, p. 371.

consideration circumstances which render that application inconsistent, as Dr. Milner acknowledges, with the facts. When a stream of water strikes a plane opposed to it, a small number only of the particles of the water touch the plane, and unless we suppose these particles to be pressed forward by the water which is behind them, the actual pressure exerted against the plane cannot be accounted for. But that action of the water is not considered in the prevailing theory; and it is omitted even in the corrected theory which has been proposed by M. de Borda and Mr. Waring;—they appear not to have considered, that when the number of planes acted upon are increased, the quantity of water acting upon each plane is decreased in the same proportion; neither are the number of planes acted on in a given time "inversely as the relative velocity," as stated by Mr. Waring.

The Edinburgh reviewers object to Mr. Smeaton's opinions, upon more general grounds, at pages 126—7—8, and continuing to reason as if he had understood the consideration of time to be necessarily excluded in all [illegible] of force, they truly and eloquently observe, that "in most instances, time is a very [illegible]erial element in the estimation of an [illegible]

kind; and is, of all our resources, that which it most behoves us to economize."*

Now, I apprehend, it is obvious, from the whole of Mr. Smeaton's reasoning on this subject, that he was perfectly aware, that, in most cases of moving force, if the pressure, the time, and the *manner* of its acting be given, the effects may be found. He observed, however, (as in the two first cases) that the effects were not always in proportion to the pressure and the time of its acting. But he found, that if the pressure and the space through which it acts (or when variable, the fluent of the pressure into the space) be given, the effects may always be determined, without reference to the *manner*, or the time, in which they may be produced; and finding the total amount of the effects to be, in all cases in proportion to the product of the pressure multiplied by the space through which it acts, whatever may be the *time* or the *manner* of its acting, he considers that product to be the principle capable of the most general application, and consequently adopts it as the proper measure of mechanical force.

With regard to the [illegible] of t[illegible]e,
I have al[illegible]
was fully [illegible]
exemplary [illegible]

its various bearings. We can form no notion of velocity, without taking time as an element of it.—As far as it relates, however, to mechanical power, time would come under his consideration chiefly in the following manner. If, for example, the object before him was to apply, to the best advantage, a given stream of water in producing a mechanical effect, he would first ascertain the quantity of water passing in any given time, and the height of its fall. He would next inform himself whether the effect to be produced should be continuous or intermitting in its duration. If continuous, he would construct his machine of such dimensions as to receive and apply the power of the stream uniformly and constantly from hour to hour, and from day to day. But if it were required to produce an intermitting effect, he would construct his machine of larger dimensions, in order to avail himself of the quantity of water which might be reserved during the time that no effect was required to be produced; and he would take care to arrange and proportion the whole, so that no more people than necessary should be employed in attending it. In the latter case, the machine would be said to be more powerful than in the former: but the word power, when used in that sense, has no reference to the

measure of the effect when compared with the force by which it is produced. The machine, without the moving force, has no power; and when we speak of the greater or less power of a machine, we only mean to say that we make use of a larger or smaller instrument to convey the moving force. If we have to let off the water from a reservoir, we know that it will be emptied in less time through a large aperture, or channel, than through a small one; and just so we know, that by a large and strong machine, a given quantity of moving force may be conveyed in less time than by a small and weak one. But if the whole, or any determinate portion of the moving force be properly applied, the whole, or proportionate effect, must nevertheless be the same, whatever may be the portion of time occupied in the operation. And the same principle holds good in the application of the elastic force of steam, or of any other moving force, to produce a mechanical effect.

In objection, however, to this, the reviewers observe as follows:—

"When it is said, for example, that a bushel of good coals will give to a steam-engine the power required to grind eleven bushels of wheat, this must al mply a rate of burning in limits; for the fuel

might be applied so slowly that the steam generated would not be of strength sufficient to work the mill; or it might be made to turn so fast, that very little effect would be produced. In the same way, when Mr. Smeaton says, if 1000 tons of water be let out on an overshot wheel, and descend through twenty feet, it will grind the same quantity of corn, at whatever rate it be expended,* the extreme cases of very great slowness, or very great rapidity, must surely be excepted. But if the extreme cases must be excepted, it is a proof that, even in the intermediate cases, the effect is not constant or invariable in its magnitude, though the differences may be inconsiderable; this, at least, is what one would be disposed to infer from that continuity in the variation of causes and effects, to which there is, perhaps, no exception, either among the works of nature or of art."†

To these objections it may be replied, that however slow or quick the combustion of the coals may be, if they be effectually burnt, the full quantity of heat must be given out. If the heat be allowed to escape without being communicated to the water; or, if after being communicated to the water the pressure of the

* Philos. Trans. 1776, p. 474.

† Edinburgh Review, vol. 12. p. 129.

steam be not wholly applied in producing the intended effect, the loss must be owing to practical imperfections in the construction of the apparatus. Such imperfections must exist, more or less, in every apparatus, and they will, no doubt, be greatest in extreme cases. But although the whole heat, or the whole force, can, in practice, never be completely transferred from one given object to another, yet there can be no doubt of the real existence of both the heat and the force in their full quantities; and we can form no idea of the portion of time being limited in which the one must be evolved or the other transferred.

A water-wheel may be made to move with a velocity so great, that almost the whole pressure of gravity shall be employed in generating motion in the *water;* or it may be made to move so slow as to require a wheel of such magnitude to hold the water, that almost the whole of the force shall be exhausted in generating motion in the wheel, and in overcoming the friction of the machine; but the whole moving force is, nevertheless, in both cases exerted, and it is immaterial to the principle of its proper measure, whether it be applied in generating motion in the water, or in the machine,—in overcoming friction, or in producing any other known effect of moving force.

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rectilinear force. That inconsistency (stated in case 3d) is overlooked in the usual demonstrations respecting rotatory motion; it is nevertheless one of considerable importance, and it requires explanation. I have already endeavoured to show (p. 139.) that the explanation, which refers us to the properties of the lever, is by no means sufficient. If, however, the product of the mass into the square of its velocity, be taken as the proper measure of the force of a body in motion, the explanation is obvious.

The case of the balance beams (case 4th.) has been adduced by many authors in proof of the moving forces being as the masses multiplied into their velocities. There is no doubt that after they have been put in motion, the weights will balance each other the same as when they were at rest; but the question is, whether or not the motion of *n* can be generated by a moving force no greater than that which generates the motion of *m*? If these two quantities of motion can be generated by equal forces, the same forces should generate equal quantities of motion in *o* and *p*; but equal pressures applied to A and C will not produce, in equal times, equal quantities of motion in the respective weights. Mr. Emerson, by neglecting this circumstance, appears

If it appear that I have insisted too much on this part of my subject, it should be recollected that many of the objections which I have been endeavouring to meet, apply not only to the particular cases under consideration, but generally to the whole question at issue. I must acknowledge too, that I have felt more than ordinary solicitude that the experience and the conclusions of one who has long been looked up to, in this country, as the father of civil engineers, should be duly appreciated.

But it is not necessary, I apprehend, to resort to complicated cases for the purpose of examining the points in question. If the two first cases which I have stated, were once distinctly explained and agreed upon, no difficulty would remain in explaining their various and multiplied applications in machinery.

Although these cases comprehend much of what relates, in this question, to rotatory motion, the three following cases apply more particularly to that branch of the subject.

In rotatory motion, it is universally admitted, that four times the force is necessary to generate the same angular velocity, or twice the absolute velocity, in the same body placed at twice the distance from the centre of motion; and it is but reasonable to enquire why we must have one measure for rotatory, and another for

rectilinear force. That inconsistency (stated in case 3d) is overlooked in the usual demonstrations respecting rotatory motion; it is nevertheless one of considerable importance, and it requires explanation. I have already endeavoured to show (p. 139.) that the explanation, which refers us to the properties of the lever, is by no means sufficient. If, however, the product of the mass into the square of its velocity, be taken as the proper measure of the force of a body in motion, the explanation is obvious.

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its centrifugal force from the centre of the earth; but that exception does not apply to the case in question, for the centrifugal force, whatever it is, must, according to M. du Buat's theory, be added to the non-pressure. In confirmation of his theory of non-pressures, M. du Buat observes, "Qu'ayant fait mouvoir, à une certaine profondeur, dans une eau stagnante, un tube vertical ouvert par les deux bouts, dont le supérieur étoit hors de l'eau, le fluide s'est maintenue dans le tube, plus bas que la superficie du réservoir, d'une quantité à-peu-près égale à la hauteur dûe à la vitesse avec laquelle il étoit mu."* But he has omitted to take into consideration the cohesion or the lateral action of the particles of the water upon each other, which has since been so well observed by M. Venturi; from whose experiments, and from those of Dr. Matthew Young,† made under the receiver of an air-pump, we may safely conclude, that, were it not for the pressure of the atmosphere, and the cohesion of the particles, there could be no depression in the tube as observed by M. du Buat; and, had he been aware of these circumstances, he surely would never have reasoned as he has done on the subject of

* Principes d'hydraul. Vol. 2. p. 156.

† Irish Philos. Trans. vol. 7. p. 63.

non-pressures. But to return to the subject of water-wheels.

It has been attempted to be theoretically demonstrated by M. de Borda, and afterwards by Mr. Waring, of America, that the force of the water against the wheel is not proportional to the square of the velocity with which it strikes the wheel, but that it is in the simple ratio of that velocity; and that the maximum effect is therefore produced when the velocity of the wheel is half that of the stream.

M. de Borda, in reference to the labours of others, says, "On ne considéroit qu'une seule palette contre laquelle on cherchoit la force du choc du fluide;---mais il falloit observer que dans le mouvement dont il s'agit, l'action du l'eau ne s'exerce pas contre une palette isolée, mais contre plusieures palettes à la fois, et que ces palettes fermant tout le passage du petit canal et ôtant au fluide la vîtesse qu'il a de plus qu'elles, la quantité du mouvement perdu par ce fluide, et par consequent le choc qu'éprouvent les palettes, n'est plus proportionnelle au carré de la difference des vîtesses du fluide et des palettes, mais seulement à la difference de ces vîtesses."*

* Memoires de l'Acad. Paris, 1767, p. 274.

Mr. Waring's demonstration is as follows: "If the relative velocity of a fluid against a single plane be varied, either by the motion of the plane, or of the fluid from a given aperture, or both, then, the number of particles acting on the plane in a given time, and likewise the momentum of each particle, being respectively as the relative velocity, the force on both these accounts, must be in the *duplicate* ratio of the relative velocity, agreeably to the common theory, with respect to this *single plane*; but, the number of these planes, or parts of the wheel acted on in a given time, will be as the velocity of the wheel, or *inversely as the relative velocity*; therefore the moving force of the wheel must be in the simple direct ratio of the relative velocity," and, consequently, the maximum effect must be produced when the velocity of the wheel is half that of the water.*

But this kind of demonstration cannot, I think, be very satisfactory. It leads, I apprehend, to this conclusion, that we may double the power of any undershot water-wheel, (whatever may be its velocity) by merely doubling the number of its floats or planes acted upon by the water. Mr. Smeaton, how-

* American Philos. Trans. vol. 3. p. 146.

of two parts, as described in the figure, having the ratio to each other of $GF^2 : KI^2$. If the pressure be not uniform, the fluent of the pressure into the space will bear the same relation which D H bears to the sum of the products of the masses into the squares of their velocities.

I am quite at a loss to understand why Mr. Atwood excluded this case from those in which the moving force may be estimated by the products of the masses into the squares of their velocities. If, in cases of rotatory motion about fixed axes, that principle "obtains," as he observes, "without exception," there can, I think, be no exception to its application in cases of this description.

Having gone through the examples of force producing motion from a state of rest, we come now to the examination of cases where motion is destroyed, or where it is transferred from one body to another.

It was a favorite doctrine with the Cartesians, and it was maintained also, though upon quite different principles, by Leibnitz, and John Bernoulli, that motion could not be lost; for the same quantity of motion, or of force, it was said, must be always preserved in the world. A similar doctrine, applied to explain

immersed three inches under the surface, and found that it supported a weight of 19.45 liv. which, by the theory, should have been only 8.75 liv.* M. de Prony attempts to account for the results obtained by Don Juan, by the additional pressure occasioned by the surface of the water over the plane being raised higher than the general level of the current. That circumstance, however, can account for a small part only of the difference. M. du Buat explains his experiments by his theory of non-pressures, which I have already shown to be fallacious.

M. du Buat has described other experiments which are considered by some to accord better with the theory.† They were made upon insulated veins of water, spouting from the perpendicular side of a vessel against a surface not greater than the section of the vein; and from their results he draws the following conclusions: "Il résulte des expériences qui précèdent, que le choc d'une colonne ou d'une veine fluide contre une surface de même étendue & directe, est sensiblement égal au produit de cette surface, par la hauteur dûe à la vitesse. L'intensité du choc dépend néanmoins en partie de la liberté plus au moins

* Principes d'hydraul. vol. 2. p. 218. † Ibid, p. 142, &c.

grande que les filets ont de se dévier aux approches de cette surface; mais si la veine rencontre une surface plus grande qu'elle, qui l'oblige à changer en entier la direction de tous ses filets, la vitesse perdue, étant par là augmentée, la resistance devient beaucoup plus grande."*

But in these experiments, a part only of the vein strikes the surface opposed to it, and the force of that part appears to be equal to the force assigned by the theory to the whole vein.

Of all theoretical propositions, that which was first demonstrated by Daniel Bernoulli in his Hydrodynamics, page 290, and afterwards more fully by the same author, in the Comment. Petropol. vol. 8. page 120, appears to be the most applicable to Mr. Smeaton's cases, and comes the nearest to his results. It is, that, when the force of an insulated vein of water is directed perpendicularly against a plane indefinitely large, its pressure against the plane is equal to the weight of a column of water, of which the base is equal to the area of the section of the vein, and the height equal to twice the height due to the velocity of the vein. But the circumstances of this case are not quite the same as those of Mr. Smeaton, and he found the pressure against

* Principes d'hydraul. vol. 2. p. 150.

the plane to be still greater than the weight of a column of twice the height due to the relative velocity of the water and the wheel.

The most important conclusions drawn by Mr. Smeaton from his experiments are (as I have already noticed) not in his maxims; but they are to be found, I apprehend, in the two following observations, which I shall quote in his own words:

1. "It is somewhat remarkable," he says, "that though the velocity of the wheel in relation to the water turns out greater than $\frac{1}{2}$ of the velocity of the water, yet the impulse of the water, in the case of a *maximum*, is more than double of what is assigned by the theory.*

2. "We have seen before, in our observations upon the effects of undershot wheels, that the general ratio of the power to the effect, when greatest, was 3:1; *the effect, therefore, of overshot wheels, under the same circumstances of quantity and fall, is at a medium double that of undershot:* and as a consequence thereof, *that non-elastic bodies, when acting by their impulse or collision, communicate only a part of their original power;* the other part being spent in changing their figure in consequence of the stroke."†

* Philos. Trans. 1759, p. 113. † Ibid, 1759, p. 130.

It was chiefly in this last consideration that he found the prevailing theory to be defective; for, according to that theory, as it is applied in explaining the collision of bodies, there can be no force spent in producing change of figure: and it is very remarkable, that no succeeding writer has, as far as I can learn, paid any attention to this circumstance.

However much Mr. Smeaton's valuable observations may have been disregarded by authors, they have not been lost to practical men. Before the publication of the paper which I have been endeavouring to defend, several mills had been constructed under Mr. Smeaton's direction, in which his chief object was to apply the water so that less of its force should be expended in producing change of figure, and consequently more of its force be communicated to the wheel. Although he had obtained by his experiments results which were "more than double of what is assigned by the theory," yet by comparing the *effective* with the real head, he found that nearly half the power was, in many instances, spent in producing a change of figure in the water, before it reached the wheel; and still finding (as stated above in the second observation) that more than half of what remained of the

power was spent in the same way, by the manner in which it acted upon the wheel; he determined to apply the water, in all cases, so that it should act more by its weight, and less by its impulse; and the advantage gained by that improved construction was found to be fully equal to his expectations. It was afterwards so generally adopted and improved upon by himself and by other engineers in this country, that although undershot water-wheels were, about fifty years ago, the most prevalent, they are now rarely to be met with; and wherever the economy of power is an object, no new ones are made. So that all the points in question, as far as they relate to undershot water-wheels, although highly important at the time when Mr. Smeaton wrote his first paper, are now become matters of mere speculative curiosity, and, in this country at least, they can no longer be of any practical use. The question, however, respecting that part of the power which is expended in producing a change of figure, is highly interesting in other points of view, and we shall have occasion to consider it more fully when we come to examine the 6th, 7th, 8th, 9th, 12th, and 13th cases.

Dr. Milner, in allusion to Mr. Smeaton's remarks on the theory, observes that, "It is

acknowledged, that the experiments which have been made to determine the effects of wind and water-mills do not agree with the computations of mathematicians; but this is no objection to the principles here maintained. Writers generally propose such examples with a view rather of illustrating the methods of calculation by algebra and fluxions, than of making any useful improvements in practice. They suppose the particles of the water to move in straight lines, and to strike the machine with a certain velocity; and after that to have no more effect. As such suppositions are evidently inconsistent with the known properties of a fluid, we are not at a loss to account for a difference between experiment and theory; and therefore it should seem unreasonable to assert, that certain authors of reputation have neglected the collateral circumstances of time, space, or velocity in the resolution of these problems, unless we are able to point out such omissions."* But if the theory be applicable to speculative objects only, why are its conclusions laid down as rules to be adopted in practice? Mr. Smeaton objected to the practical application of the theory by the distinguished authors which he quoted, because they omitted to take into

* Philos. Trans. 1778, p. 371

consideration circumstances which render that application inconsistent, as Dr. Milner acknowledges, with the facts. When a stream of water strikes a plane opposed to it, a small number only of the particles of the water touch the plane, and unless we suppose these particles to be pressed forward by the water which is behind them, the actual pressure exerted against the plane cannot be accounted for. But that action of the water is not considered in the prevailing theory; and it is omitted even in the corrected theory which has been proposed by M. de Borda and Mr. Waring;—they appear not to have considered, that when the number of planes acted upon are increased, the quantity of water acting upon each plane is decreased in the same proportion; neither are the number of planes acted on in a given time "inversely as the relative velocity," as stated by Mr. Waring.

The Edinburgh reviewers object to Mr. Smeaton's opinions, upon more general grounds, at pages 126—7—8, and continuing to reason as if he had understood the consideration of the time to be necessarily excluded in all estimations of force, they truly and eloquently observe, that "in most instances, time is a very material element in the estimation of an effect, or an event of any

kind; and is, of all our resources, that which it most behoves us to economize."*

Now, I apprehend, it is obvious, from the whole of Mr. Smeaton's reasoning on this subject, that he was perfectly aware, that, in most cases of moving force, if the pressure, the time, and the *manner* of its acting be given, the effects may be found. He observed, however, (as in the two first cases) that the effects were not always in proportion to the pressure and the time of its acting. But he found, that if the pressure and the space through which it acts (or when variable, the fluent of the pressure into the space) be given, the effects may always be determined, without reference to the *manner*, or the time, in which they may be produced; and finding the total amount of the effects to be, in all cases in proportion to the product of the pressure multiplied by the space through which it acts, whatever may be the *time* or the *manner* of its acting, he considers that product to be the principle capable of the most general application, and consequently adopts it as the proper measure of mechanical force.

With regard to the proper economy of time, I have always understood that Mr. Smeaton was fully sensible of its value, and most exemplary in his punctual attention to it, in all

* Edinburgh Review, vol. 12. p. 128.

EFGH and the bodies are supposed to be put in motion before the spring is disengaged, there is, according to the prevailing theory, no motion produced by the spring.—There is merely a transfer of motion from C to D, and we have only the same motion after, that we had before, the action of the spring.—Is there not some inconsistency in supposing the spring to produce motion in one case but none in the other?

If instead of the unequal pressure of a spring, an uniform pressure be applied, as in the 8th case, the various quantities of mechanical force expended at different periods of the operation, will be more distinctly shown: for, the pressure being constant, each portion of space through which it acts will express the quantity of mechanical power which has been expended in that space.

In its passage through a space $=EH=\frac{3}{4}EF$ (Fig. 8) an uniform resistance has been opposed to A, which would bring it to rest in a space $= EF$. When it has arrived opposite to H it has therefore lost half its velocity; and B having arrived opposite to I by the action of an equal pressure through a space $= FI = \frac{1}{4}EF$, has acquired the velocity $\frac{v}{2}$; and KH, equal $\frac{1}{2}EF$, will consequently be

the depth of the penetration of c into A. Now if A be a nonelastic soft mass, of clay for example, we know that it cannot be penetrated without force; nor have we any reason to suppose that the force which has been expended in producing the penetration, can ever be restored. We therefore cannot expect to find in the motion of A and B after collision, the same quantity of force which they had before collision. If, however, the pressure into the space through which it acts, be taken as the measure of the force, we shall find, that a compound effect, has been produced by A in its passage through the space = E H, that only $\frac{1}{3}$ of the force which A has lost has been communicated to B, and that the other $\frac{2}{3}$ of that force has been spent in producing a change of figure in A. These proportions are obvious from the mere inspection of the diagram. We may suppose A to be a much harder substance than clay, so that the space represented by EF may be very small; but the pressure being proportionally greater, the product of the pressure into the space will still be the same, however small the penetration may be.

Any explanation, however, which takes into consideration the force which is expended in producing a change of figure, is strongly

objected to by all those who hold that the product of the mass into its velocity is the proper measure of the force of a body in motion. They contend that "all the experiments which are usually brought to determine the impressions made upon soft bodies, as snow, clay, &c. are absolutely unfit for the purpose." That "the circumstances, which take place in the production of these effects, are such as we can never discover." And that "the directions in which the particles recede, the velocities they acquire, their mutual actions upon one another, and lastly, the time, in which these effects are performed, are all beyond the reach of computation." *

To this it may be replied, that if only the pressure and the space through which it has acted be determined, it would be quite superfluous to enter into any farther computation of the circumstances above enumerated, in order to estimate the quantity of mechanical force expended in producing the impression. For, whatever may have been the relative directions, velocities or mutual actions of the particles during the time that the impression was making, no internal motion remains after the impression is completed; and the force

* Dr. Milner. Philos. Trans. 1778. p. 353.

can have been spent in no other way than in compressing the particles together, or in overcoming their tenacity. To take a familiar example.—If a quantity of corn is to be ground, a considerable quantity of motion must, no doubt, be produced before that can be effected;—but after it is ground, there is no more motion in the flour than there was in the corn before it was ground, and the whole force employed must have been expended in overcoming the tenacity or cohesion of the particles of the corn.

In answer to the very common objection, that the quantity of force expended in producing an effect of this kind, cannot be precisely ascertained, it may be observed, that in real practice, such quantities of force are estimated with quite as much precision as the force necessary to generate a given velocity in a given mass,—in projecting a cannon ball, for example.—The application and measurement of mechanical force producing changes of figure are indeed the chief occupations of practical men, in the construction and management of machinery.

The force spent in producing change of figure in the collision of bodies, was noticed by John Bernoulli in his dissertation De vera notione virium vivarum, as follows. "Si

corpora non sunt perfecte elastica, aliqua pars virium vivarum, quæ periisse videtur, consumitur in compressione corporum, quando perfecte se non restituunt; a quo autem nunc abstrahimus, concipientes, compressionem illam esse similem compressioni elastri, quod post tensionem factam impediretur ab aliquo retinaculo, quo minus se rursus dilatare posset, et sic non redderet, sed in se retineret vim vivam, quam a corpore incurrente accepisset: unde nihil virium periret, etsi periisse videretur." *

From this passage, and from various other passages in his works, relating to the doctrine " de conservatione virium vivarum," it appears, that Bernoulli thought it necessary to maintain that no force could be lost, and that even in the collision of nonelastic bodies he considered the change of figure to be such, that the force which had been expended in producing it might be recovered by the restoration of the figure, or by some other means. Why he considered it incumbent upon him to maintain such opinions, or upon what foundation he understood them to rest, it is hard to say. Experience furnishes us with nothing which can justify the conclusion that the force

* Bernoulli's works, Vol. iii. p. 243.

spent in producing change of figure in non-elastic bodies, can ever be restored.

I believe Mr. Smeaton was the first who subjected to actual admeasurement the force spent in producing change of figure in the collision of non-elastic bodies.* He appears to have been led to this investigation, not by curiosity merely, but by a conviction of the insufficiency of the prevailing doctrines of forces to account for the facts which were constantly presented to him in his ordinary occupations, and particularly, as I have before observed, in the action of water on water wheels. It is very remarkable, that while Mr. Smeaton's other dissertations on the principles of moving force, have met with considerable attention abroad as well as at home, this last treatise on the collision of bodies, (which he himself considered a most important one, as containing the best confirmation of his former conclusions) has been almost totally neglected by all succeeding writers. It is impossible for me to do justice to it by giving an abstract of it; but I would earnestly recommend the entire treatise to the attention of all those who take an interest in investigations of this kind.

With regard to the collision of bodies which are supposed to be perfectly hard as well as

* Philos. Trans. 1782.

nonelastic, Mr. Smeaton understood a contradiction to be involved in the very supposition of the existence of such bodies. It has never been contended that any such are to be found in nature. But it is very generally argued, with Mr. Maclaurin, that "there is the same objection [of non-existence] against admitting and treating of bodies of a perfect elasticity.*" In reply to this I would observe that, the objection does not appear to be of the same weight against perfectly elastic, as against perfectly non-elastic hard bodies. For, we have substances which approach very nearly to perfect elasticity; but we can find no substance of which the qualities approach to hardness and non-elasticity united. In general the elasticity encreases as the hardness encreases, and no substance has ever been produced that can be called hard, without possessing, at the same time, great elasticity.

It does not appear that the possible existence of a perfectly hard non-elastic body was obvious to the first discoverers of the laws of percussion. Huygens appears to have understood a hard body to be one that is perfectly elastic. His 6th law of percussion is as follows, "Summa productorum factorum a mole cujuslibet corporis *duri* ducta in quadratum

* Account of Sr Isaac Newton's discoveries, p. 93.

suæ celeritatis, eadem semper est ante et post occursum eorum." *

M. Laplace considers that " Ce principe " de la conservation des *forces vives* " n'a lieu que dans les cas, où les mouvemens des corps changent par des nuances insensibles. Si les mouvemens éprouvent des changemens brusques, la force vive est diminuée d'une quantite que l'on determinera de cette manière;"——† and taking it for granted, in the usual way, that where the change of motion is *sudden*, the bodies must be non-elastic, he investigates the motions which are known to result from the collision of non-elastic soft bodies. But that conclusion is not justified by experience; for the characters of elasticity are often the most apparent where the changes of motion are, as far as we can judge, the most sudden.

The supposition of the possible existence of a perfectly hard body, appears to involve another inconsistency which I will endeavour to state in a few words.—The resistance, or pressure, against *c* (fig. 8) being encreased, and the depth of its penetration being diminished, in proportion as the hardness of A is increased, it follows, that if, by supposing A to be perfectly hard, the depth of the pene-

* Phil. Trans. 1669. p. 928.

† Méchanique céleste, vol. 1. p. 52.

tration be reduced to nothing, the pressure must be increased to infinity. That is, the pressure must be infinitely great to communicate even the smallest finite quantity of motion. But I believe the "law of continuity" is not so much objected to now as it was formerly, and few will be disposed to contend that a body may, from a state of rest, arrive at any given velocity, without passing through the intermediate degrees of velocity, between that and rest; and consequently, few will now contend for the possible existence of a perfectly hard substance.

If instead of a non-elastic soft substance, we suppose A to be a hollow sphere filled with a dense elastic fluid, and *c* to pass through a hole in the side of the sphere so as to move without friction, and be uniformly pressed outwards by the fluid; A will then represent a perfectly elactic body.

It may be proper to observe, that although we suppose *c* to make no penetration into B, we do not suppose B to be perfectly hard. We only suppose it to be so much harder than A that the penetration shall be very small when compared with the penetration into A. If we were to suppose A and B to yield equally to *c*, the same explanation of the phenomena, as when A only is supposed to be penetrated,

will strictly apply; only the diagram would be a little more complicated.

Let us now suppose the first part of the operation in the collision of A against B, to be the same as already described in the case of a soft body, and supposing them to be in the situation as represented at No. 2, let us observe what must follow.—When A has arrived opposite to F, as represented at No. 3, *c* will have returned to its original place with respect to A, and B will have arrived opposite to G (FG being = EF), A will be at rest, and B will have acquired the full velocity *v*.—Now it is obvious, that if A had not moved on from its position No. 2, *c* would in this last part of the operation, have acted upon B only till it arrived opposite to L (FL being $= \frac{3}{4}$ EF), and its final velocity would have been only $\sqrt{\frac{3}{4} v^2}$. But A having moved on to its place No. 3, *c* will have acted on B till it has arrived opposite to G; and the force which has been lost by A in its passage through the space = HF, as well as the force of *c* through a space = HK, has been communicated to B. In other words,—the force which, in the first part of the operation, had been expended in producing the change of figure, has, in the last part of the operation, been reproduced by the expansion of the

figure to its original state, and has, together with the remaining force of A, been communicated to B. If this explanation be applied to the change of motion produced in C and D Fig. 7, as referred to at page 185, it must be obvious, I think, that when C is brought to rest, the force which it has lost, and the force of the spring, have both been communicated to D.

In the collision of unequal masses, the distribution of the force is rather more complicated. Let M (fig. 15) be immoveable and filled with a dense elastic fluid so that N, moving with the velocity v and meeting with an uniform resistance, would be brought to rest by driving the cylinder C up to O. Then if we suppose M, $= 2$N, to be in free space, and if we divide O P, $=$ O R, into nine equal parts, and make O S $= \frac{1}{9}$ O R, it will be obvious, that when N has arrived at S′ its velocity will be $\frac{v}{3}$, and M will at the same time have arrived at 2′ and will have acquired the velocity $\frac{v}{3}$, and the penetration of C into M will be $\frac{2}{9}$ O R.—In this part of the operation then, N has (on the principles adopted in explaining the last case) lost, or rather given out, $\frac{8}{9}$ of its force; of the effects of which $\frac{2}{9}$ are found in the acquired motion of M and $\frac{6}{9}$ in

the change of figure of M. In the next stage of the operation N will have arrived at O′ and be at rest; M will have arrived at 4.5, and will have acquired the velocity $\frac{v}{2}$. And lastly when M has arrived at 8′ it will have acquired the velocity $\frac{2}{3}$ v, and N will have moved back to S″ and will have re-acquired the velocity $\frac{v}{3}$, and the balls will be at the same distance that they were at first when N struck C.—In explaining these facts by the common theory, it is admitted that N has communicated to M a greater quantity of motion than it had; that inconsistency, however, is supposed to be removed by saying, that the motion of N being in the contrary direction, it must be deducted from the motion of M, and the remainder will be equal to the original motion of N. But we know that a body cannot be put in motion, in any direction, without force, and as the final motion of N, as well as that of M, must have been derived from the original force of N; it appears that the motion of N should be added to, instead of being deducted from, the motion of M, before we can properly compare the effects with the force by which they have been produced.—If N had remained at rest at O′, M would have been acted upon by C till it arrived at 9′, and the whole

original force of N would have been found in the motion of M, which would finally have acquired the velocity $\sqrt{\frac{v^2}{2}}$.

This last explanation is given by Dr. Wollaston as follows. "But there is one view," he observes "in which the comparative forces of impact of different bodies was not examined by Smeaton, and it may be worth while to shew that when the whole energy of a body A is employed without loss in giving velocity to a second body B, the *impetus* which B receives is in all cases equal to that of A, and the force transferred to B, or by it to a third body C, (if also communicated without loss and duly estimated as a mechanic force,) is always equal to that from which it originated.

"As the simplest case of entire transfer, the body A may be supposed to act upon B in a direct line through the medium of a light spring, so contrived that the spring is prevented by a ratchet from returning in the direction towards A, but expands again entirely in the direction towards B, and by that means exerts the whole force which had been wound up by the action of A, in giving motion to B alone."*

* Philos. Trans. 1806. p. 19.

In the explanations which I have offered of the phenomena which occur in the collision of bodies, I have supposed all the changes of motion and of figure to be gradual, not instantaneous; and it may be objected to these explanations that they cannot be applied to cases of instantaneous impact. But I believe it is now generally admitted, as I have already observed, that impact cannot be perfectly instantaneous,—that some small but finite portion of time must pass during the operation;* and if this be so, the changes of motion must occupy also some portion of space.—Now if we suppose that portion of space to be magnified by means of lenses, we cannot doubt that we should see all the changes of figure, as well as of motion, distinctly in their order, the same as they actually appear when they are gradually produced in extended spaces, and the same explanations may be strictly applied to the changes which take place in the smallest as well as in the largest spaces.

The 9th case is stated merely to show, that we cannot form a just estimate of the forces of bodies in motion by attending solely to the *quantity of motion* of their common centre of gravity; and that, in cases of composition of motion, wherever there is a loss of mechanical

* See Hutton's Dict. art. Force, vol. 1. p. 496.

force *in any direction*, there must be a corresponding *change of figure*, which may always be estimated upon the principles adopted in the preceding cases.

In the 10th case, the *quantity of motion* of A (fig. 10) after collision is the same as that of the common centre of gravity of E and F before collision. But the whole forces of E and F are not exhibited in the *quantity of motion* of their common centre of gravity.—The motion of A, however, is the whole effect produced, and if we estimate its force by its mass into its velocity, we cannot account for the total loss of the forces of E and F; but if we estimate all the forces by the masses into the squares of their separate velocities, the agreement between the forces and their joint effect is obvious.

I have already adverted (page 134) to a statement of a case of composition of motion made by M. Laplace, in which a hypothetical relation of the force of a body in motion to the square of its velocity is adopted, and where the supposed effects would be quite at variance with those of experience. It will perhaps be better understood with a reference to this 10th case.

M. Laplace says, " La force peut être

exprimée par une infinité de fonctions de la vîtesse, qui n' impliquent pas contradiction. Il n'y en a point, par exemple, a la supposer proportionnelle au carré de la vîtesse. Dans cette hypothèse, il est facile de déterminer le mouvement d'un point solicité par un nombre quelconque de forces, dont les vîtesses sont connues; car si l'on prend sur les directions de ces forces, à partir de leur point de concurs, de droites pour représenter leurs vîtesses, et si l'on détermine sur ce mêmes directions, en partant du même point, de nouvelles droites qui soient entre elles, comme les carrés des premières; ces droites pourront représenter les forces elles-mêmes. En les composant ensuite par ce qui précède, on aura la direction de la résultante, ainsi que la droite qui l'exprime, et qui sera au carré de la vîtesse correspondante, comme la droite qui représente une des forces composantes, est au carré de sa vitesse. On voit par là, comment on peut déterminer le mouvement, d'un point, quelle que soit la fonction de la vîtesse qui exprime la force."*

Now if AB (fig. 10th) be produced to G, and AC to H, making $AH : AC^2 : : AG : AB^2$, and if we complete the rectangle, and

* Système du Monde, p. 141.

draw the diagonal AI; we shall have a diagram of the construction described above by M. Laplace; and, if I understand him right, he concludes, that if the forces of E and F are respectively as the squares of their velocities, AI must be the resulting direction of A, and the square of its velocity must be to AI as $AB^2 : AG$. If, by the force of a body in motion being as the square of its velocity, it were meant, that the pressure exerted in bringing it to rest in a *given time* must be as the square of its velocity, the result must no doubt be such as M. Laplace describes. I cannot find, however, that this meaning has ever been applied to the principle in question. Such a hypothesis could not be entertained, indeed for a moment, without setting aside the incontrovertible explanations and conclusions of Galileo. In answer to the objection implied, in the reasoning of M. Laplace, against the force being as the square of the velocity, I can only repeat, what I have already so often repeated, that it is not the pressure exerted in a *given time*, but the pressure exerted through a *given space*, that is understood to be universally as the mass into the square of its velocity; and I may add that there is nothing hypothetical in this conclusion.—Being derived from an

induction of facts, it must stand or fall with the facts on which it is grounded.

In the next case, where the angle BAC (fig. 11) is not a right angle, the results after collision are, in two respects, different from the last. E and F are not at rest after collision; and the *quantity of motion* of A is not the same as that of the common centre of gravity of E and F before collision.* This case, or rather the converse of it in a less simple form, was first explained by John Bernoulli in the eleventh chapter of his "Discours sur le Mouvement," and the solution which I have given (page 123) will be found to agree with his. In his twelfth chapter, however, he extends his solution to the case where a ball D (fig. 16) strikes any number of pairs of balls,—the balls in each pair being equal and at equal distances from the line of direction of the striking ball.—But that solution, as it has been justly observed by Mr. Robins, "will be true only when the same time is taken up in communicating

* In describing this case at page 123, I have omitted to state that E and F are supposed to move with equal velocities; but it will be obvious from the figure and from the results which are given, that it was so understood.

motion to all the balls," * and that cannot take place unless a peculiar modification of the elasticity be adapted to the respective masses and positions of each pair of balls at their points of contact; and even then the results will not always be as they are laid down by M. Bernoulli. His solution therefore was not, what he understood it to be, a general one.

Cases of this description appear to have been imperfectly understood at the time when M. Bernoulli wrote. In the "Histoire de L'Academie Royale" of Paris, for the year 1721, p. 84, the following case is stated. Two equal balls moving with equal velocities are supposed, as in the 11th case, to strike at the same instant a third ball at rest; and the directions AC and AB of the striking balls E and F are supposed to be such that we shall have AC or AB=2 AH. That is, that the absolute velocity of E or F, before they strike A, shall be equal to twice the velocity of their common centre of gravity.—And it is concluded that AD will represent the velocity of A after the stroke.

It appears also that some of the most ob-

* Robins' Tracts, vol. 2. p. 186.

vious effects of elasticity in the collision of bodies were as much misapprehended then as the motion of the bodies after collision. In the same department of the valuable work last quoted, for the year 1728, the same subject (sur la force des corps en mouvement) is resumed, and at page 77 there is the following statement.

" Un corps, qui a une vîtesse à parcourir d'un mouvement uniforme 1 pied en 1 minute, parcourra 2 pieds en 2 minutes, une infinité de pieds en une infinité égale de minutes; il a en soi de quoi se mouvoir éternellement, quoique sa force soit finie, il faut seulement qu'il ne rencontre point d'obstacles. Je suppose cette force telle que quand il se sera mû pendant 1 minute, toûjours appliqué à un ressort qu'il fermera à la fin, et dont la base, qui répond à l'ouverture qu'il aura eûe d'abord, ait été de 1 pied, cette force soit entiérement consumée, et je suppose ensuite qu'au lieu de ce ressort on lui en donne à fermer deux consécutifs égaux à celui-là. Il ne peut les fermer sans les appliquer tous deux l'un contre l'autre, sans réduire à rien leur base commune double de la premiere, c'est-a-dire, sans parcourir un espace de 2 pieds. Or cet espace, il ne le peut parcourir qu'en 2 minutes,

donc dans la premiere minute il ne peut avoir fermé qu' à de mi chacun des deux ressorts, et à la fin de la seconde il les aura entiérement fermés tous deux, et sa force sera consumée."

Mr. Maclaurin has given, in his Treatise of Fluxions, page 431, some ingenious solutions of the problem where two or more bodies at rest are struck at the same instant by another body moving with a given velocity in a given direction. It is remarkable, however, that the consideration of the time was omitted by him in the same way that it was omitted by M. Bernoulli; although the oversight of the latter had been pointed out by Mr. Robins fourteen years before Mr. Maclaurin published his solutions; which appear to be defective also in the following respect. The resulting motions are first given on the supposition that the bodies are hard and non-elastic, and from these results are deduced the motions which are supposed to result from the collision of elastic bodies.—But M. D'Alembert has shown that, in all cases where the bodies which are struck are not equal to each other, and similarly situated with respect to the direction of the striking body, the supposition of hard bodies leads to erroneous results with respect to

elastic ones,* and it is remarkable that the cases selected by Mr. Maclaurin are all of that description.

Far be it from me to say that the oversights of that excellent philosopher and profound mathematician, or that the omissions or oversights of any of the distinguished men to whose works I have referred, are of much importance when compared with the numerous benefits which they have rendered to science. I only wish to show that the principle, which appears to me to be capable of general and correct application, has been condemned on insufficient grounds; and the circumstance of such a man as Maclaurin having been led to erroneous conclusions by reasoning from the supposed action of hard bodies, affords the best argument for rejecting that doctrine.

M. D'Alembert appears to have been fully sensible of the difficulties which attend the solution of problems of this description; and from his general reasoning respecting them, as well as from the demonstrations of some of them which he has given, it is obvious that, without considering the pressure and the space through which it acts, as well as the time of its acting, during the *process*, if I may so

* Traitè de Dynamique, p, 234—5.

call it, of collision; the resulting velocities and directions of the bodies, after collision, cannot be determined.

I have selected the case which I have stated, (as I have selected all the rest,) as being the most simple of its kind; and the solution which I have offered is also simple; being derived from examining the pressures and the spaces through which they act in producing the motion of A.

The 12th example is stated for the purpose of showing that, in cases where quantity of motion *in one direction* forms no part of the subject to be considered, there is in the collision of non-elastic bodies a positive loss of force, in whatever way it may be reckoned, and if that loss be estimated by examining the pressures and the spaces through which they act, a change of figure, corresponding to the force which has been expended, will be found.

The 13th case was proposed to me by my friend Mr. Dalton, to whose candid encouragement I have been much indebted in the prosecution of this enquiry. It is stated in order to show that the same effect is produced by the same force, whether it act by gradual pressure or by sudden percussion.—If the piece of clay be placed so near to A as to touch the prism when it begins to fall, the

whole impression will be produced by gradual pressure.—In estimating the force in this case, a practical man thinks of nothing but the quantity of mechanical force—or the pressure into the space—necessary to raise the prism to the given height; and as the same quantity of force will always raise it to the same height, he concludes that the same effect must always be produced by its fall, although the times in which these equal effects are produced may be very different. If instead of a piece of clay, we place a much harder substance—a block of iron for example—under the prism, we shall have an impression produced on the prism as well as on the block; and, unless the centre of motion be of a very permanent kind, we shall, when the block is placed near to A, have a change of figure in that centre also. But still if all these changes of figure could be accurately measured, by the pressure and the space expended in producing each of them, their sum would be equal to the whole change of figure produced on the clay, or on any other comparatively soft substance, placed under P. There are many very complicated cases of this kind,—such as the hammering and rolling of metals, which may, I apprehend, be all distinctly explained upon the same principles.

In the 14th Case the same effects are produced by percussion, which, in the 5th case, are produced by gradual pressure through sensible spaces; and we must either admit that the moving force of D (fig. 14) is greater than that of C, or conclude that the rotatory motion is produced without force. It may be said that there is in both cases only the same quantity of motion in *one direction*.—I must observe however, that Sir Isaac Newton understood the *sum of the motions* of the two bodies to include the rotatory as well as the progressive motion. "If two globes," he says, "joined by a slender rod, revolve about their common centre of gravity with an uniform motion, while that centre moves on uniformly in a right line drawn in the plane of their circular motion, the sum of the motions of the two globes, as often as the globes are in the right line described by their common centre of gravity, will be bigger than the sum of their motions, when they are in a line perpendicular to that line."* On this passage we have the following note from Dr. Horsley. "The contrary seems to be true; that the sum of the motions will be greatest, when the rod connecting the revolving bodies is perpendicular to the right line, along which the

* Horsley's Newton, vol. 4, p. 258.

common centre of gravity is moved. But in either way the different quantity of that sum of motion, in these two positions of the rod, equally makes for our author's assertion. Of which perhaps there is yet a more striking proof in the prodigious generation of motion by the collision of elastic bodies in certain arrangements, vid. Huygens De motu corporum ex percussione." But this is obviously an oversight of the learned editor; for, if he had bestowed a little more consideration on the case as it is distinctly stated by the illustrious author, he would not, we must presume, have given a commentary so much at variance with the text.—When A is perpendicular over B, B is at rest, and A only is in motion with the velocity $2v$. The whole *quantity of motion*, when the balls are in that position, is therefore expressed in the usual way by $A \times 2v$. But when AB is in a horizontal position, the common centre of gravity of A and B is moving horizontally with the velocity v, and each ball is moving round that centre with the same velocity v. The sum of the motions, when in that position, must therefore be $\overline{A+B}.v+A.v+B.v$; and I think, it cannot admit of a doubt that Sir Isaac Newton understood the case in that light. But although the motion is exhibited in such vari-

ous quantities according to the positions of the rod; it cannot be questioned that the *quantity of force* must remain the same, under all positions of the rod—While the motion continues *uniform* there certainly can be *no variation of the force*. It appears, therefore, (as I have before observed p. 173) that Sir Isaac Newton understood, that unequal quantities of motion might be derived from the same quantity of force. It must be acknowledged that, from some expressions of Sir Isaac Newton, in alluding to this and some other cases, it might appear—if these expressions are taken individually without reference to his general doctrines, that he supposed a variation of force to take place in this case. That supposition has been noticed by M. Bernoulli with a degree of unfortunate asperity peculiar to himself, and very inconsistent, it must be confessed, with the character by which philosophical discussions ought to be distinguished. From the context, however, it is obvious, that Sir Isaac Newton could not mean the casual expressions in question to be strictly applied as relating to variation of *force* in the cases which he mentions. For, if they can be so applied, they must be indiscriminately applied to cases which have no resemblance to each other. The *force* which is expended in

overcoming the cohesion of pitch,* for example, can never be seriously compared with any supposed change of *force* in the case under consideration.—Yet, according to Mr. Bernoulli's acceptation, Sir Isaac Newton must have meant that there was in both cases the same kind of variation of *force*.

If D be a non-elastic body, we shall then indeed have a variation of the force similar to that which takes place in the motion of the pitch.—A portion of the force will be expended in producing change of figure, and the results after collision will exhibit four distinct effects of moving force, namely, a change in the progressive motion of D, a change of figure in D, a progressive motion in G, and a rotatory motion in A and B. For, D will move on with the velocity $\frac{v}{2}$, and its figure will be changed, G will move on with the velocity $\frac{v}{4}$, and A and B will revolve around G with the velocity $\frac{v}{4}$. That is, one fourth of the original force of D will remain with it after collision,—one half will have been expended in changing the figure of D,—one eighth will have produced the progressive motion of G,—and one eighth, the rotatory motion of A and B. But if these effects must

* See Horsley's Newton, vol. 4, p. 259.

be estimated by the product of the mass into its progressive velocity, the change of figure, as well as the rotatory motion, must be left wholly unaccounted for.

If the more complicated cases of this description, where the force is neither communicated in the directions of the centres of gravity nor in those of the centres of gyration, be examined on the same principles by which I have attempted to explain the fifth case and the case before us, it will be found, that the force expended in producing change of figure, added to that which is exhibited in the motion of the bodies after collision, will always be equal to the original force of the striking body.

Having stated, more fully perhaps than is consistent with the due limits of a paper of this kind, various opinions and explanations relating to the examples of force which I have offered to the consideration of this society; I wish to observe, that the terms, pressure,—force,—moving force,—momentum, &c. are used, by different authors, and sometimes even by the same author, with various mean-

ings. It is probable therefore that I may not have understood them, in all instances, in their proper, or even in their intended meaning.* I have been careful however to give, in most cases, the authors' own words; and in all cases I have given such references that any mistakes of that kind may be easily detected by those who are disposed to examine the subject.

That great misunderstandings respecting the subject under consideration have arisen from the various senses in which the terms have been taken, must be acknowledged. But it cannot, I think, be reasonably contended that the whole has been merely a dispute about words.

Soon after it had been shown by Huygens that the "ascensional force" of a body in motion is as the square of its velocity; that

* Since page 150 was printed, I have noticed that the following passage (line 17) "that the maximum effect must consequently be as $A \times c^2$" should be corrected thus "that the maximum effect of a given quantity of water must consequently be as c^2." I wish to observe also, that although the reviewers admit that there is a great difference between the theoretical conclusions and the acknowledged results of experience, they appear to consider the theory to be unexceptionable. To that I could reply only by stating at some length the difficulties which attend the application of the theory to practice.

principle was extended and brought forward in a manner very unfavorable to its general reception. It was adduced by Leibnitz* as an argument against Des Cartes; and afterwards by Bernoulli† and others, as a principle which must supplant or supersede some of the leading doctrines of the Newtonian philosophy. Great opposition was naturally excited by these last pretensions; and, as it invariably is the case in intemperate controversies, the advocates on both sides were led into many inconsistencies. It soon became quite a party question, and the prejudices against one side became so strong, that if any one ventured to consider the absolute force of a body in motion to be as the square of its velocity, he was pitied or condemned, as if he had lapsed into a dangerous heresy. It is to be regretted that these prejudices, if such they are, are not yet entirely removed. For myself I must acknowledge, it is a matter of some concern to me, that in consequence of the explanations which I have thought it necessary to adopt in endeavouring to understand this subject, I have, by some of my very good mathematical friends, whose favorable disposition it is my wish to conciliate, been considered more in the light of a perverse schismatic than in that

* Act. Erud. Lipsiæ 1686. p. 161. † Works vol. iii.

of a patient enquirer; and I entreat that the too great length of this, I fear tedious, discussion may be ascribed to my desire to merit the latter rather than the former appellation.

I cannot help thinking that if this rejected principle had been first produced, not in opposition to, but as, what I believe it really is, an extension of the Newtonian doctrines of force, it would have been zealously cultivated and might have proved highly interesting to mathematicians, as well as of essential service to practical men, in explaining those variations of force, to the useful application of which their operations are chiefly directed.

If we wish to trace the history of this measure of force to its origin, we must go back to Galileo. It was first demonstrated by him that the spaces described by heavy bodies, from the beginning of their descent, are as the squares of the times, and as the squares of the velocities acquired in those spaces; and he first distinctly explained all the phenomena of the motions of bodies uniformly accelerated or retarded by constant forces, in their simple and likewise in their compound actions. The law of continuity appears also to have originated with him.—It is most extraordinary that both Mr. Robins and Mr. Maclaurin have

spoken of this law with great disapprobation,* and that although it had been distinctly stated by Galileo, nearly a hundred years before the time they wrote against it, they considered it as a new and a visionary doctrine produced by Leibnitz or his followers, for the purpose of controverting the arguments which had been produced in support of the supposed collisions of hard bodies. Galileo appears to have been fully sensible of the importance of the law of continuity, and to have been aware also of the objections which might probably be brought against it. In his first dialogue he supposes a difficulty to arise in the mind of one of the speakers, who states it thus " Id est, quod non satis capio, cur necesse sit, ut mobile quietem deserens, et motum inclinatione naturali subiens, omnes transeat gradus præcedentis tarditatis, qui inter quemcunque certum velocitatis gradum, et statum quietis interjecti sunt:" To which the following remarkable answer is given, " Non dixi, nec ausim dicere, naturæ ac Deo impossibile esse, velocitatem illam quam dicis, immediatè conferre: sed hoc affirmo, quod id natura de facto non præstet. Si vero præstaret, ea operatio naturæ cursum exce-

* Robins' tracts, p. 174-5.——Maclaurin's Account of Sir Isaac Newton's discoveries, p. 92-3.

deret, ac proinde miraculosa foret." * This short but comprehensive argument contains every thing that can be urged in support of any of the principles which are termed laws of nature; and it is not easy to understand upon what grounds of experience or analogy this principle of continuity has ever been rejected.

The laws of uniformly accelerated or retarded motions having been demonstrated by Galileo, the same principle was extended by Newton to motions produced by varying forces, where the acceleration or retardation cannot be uniform; and in the 39th prop. of the first book of the principia, it is demonstrated, that when a body is urged in one direction by a varying force, the square of the velocity which it has acquired in any given space, measured from the beginning of its motion, will be as the curvilinear area which is formed by the aggregate of the increments of the space drawn into right lines denoting the pressures exerted at each increment.

As far therefore as the measure of force, which is composed of the pressure into the space through which it acts, can be applied to

* Dialogus de Systemate Mundi. Lugduni 1641, p. 11. This was first published at Florence in 1632.

the estimation of the forces of moving bodies, it is, properly speaking, the doctrine of Galileo and of Newton.

But we have seen that the same principle has been still farther extended, and applied to explain the phenomena of force producing changes of figure in masses of matter.

No indications of force are more constantly presented to our notice than those which consist of mechanical changes of figure.—The fabrication of every thing that is useful or convenient to us is accomplished chiefly by the application of mechanical force to produce change of figure. The grinding of corn, the expressing of oil from seed, the sawing of timber, the hammering and rolling of metals, the driving of piles,—are all examples of moving force producing changes of figure; and although, in all these cases the effects produced are of a complicated kind, yet the moving forces by which they are produced may be estimated with tolerable precision. The force expended in driving piles into the earth, has been examined by many mathematicians. In this case, the whole force of a body in motion is supposed to be expended in driving the pile, and this quantity of force is understood to be as the height from which the body falls, or as the square of its velocity.

But there appears to be a material inconsistency in this application of the prevailing theory. For, there is in fact no difference in kind between this case and the 8th case which we have before examined; although in that case there is, according to the theory, no force expended in driving the cylinder into the ball of clay. I do not see how this inconsistency can possibly be removed, but by adopting Mr. Smeaton's explanation of the collision of non-elastic bodies.

I am aware that many object to the comparison of changes of figure with changes of motion, as effects of force. Our knowledge of both, however, appears to be acquired by the same means.—They are both produced by pressure acting through some portion of space; and there appears to be no difficulty in estimating the forces by which they are produced by the same measure.

Of all the various terms that have been adopted in explaining the phenomena which we have been examining, none has been so uniformly used with the same meaning as the word *pressure*. All our notions of force appear to be derived from *pressure*, as it is perceived by the sense of touch. By balancing and comparing all other pressures with that of gravity, we obtain a common measure of

pressure. Although pressures are balanced by pressures relatively at rest, under an almost infinite variety of circumstances; their most intricate combinations are distinctly explained and estimated by the application of a small number of general principles; and upon that subject no difference of opinion exists.

If pressure be applied to a mass of matter at rest, but free to move in any direction, the mass is put in motion. But that motion of the mass implies motion of the pressure; for unless the pressure follow and act upon the mass through some portion of space, no motion can be produced. If it be clear that the motion of a mass of matter must be produced by the action of pressure through a portion of space, it is not less obvious that the mechanical compression, or the mechanical separation, of the parts of a mass of matter, must be produced by the same means; and when we speak of the resistance of inertia in one case, or of that of repulsion or cohesion in the other, we only mean that the exertion of pressure through some portion of space is necessary to overcome the resistance in either case. Although we suppose the resistance in the different cases to proceed from different causes, we find no difference in the means by which the resistance is to be overcome; and by taking

the pressure conjointly with the space through which it acts, we obtain a common measure for this description of force.

When we speak, therefore, of the *force* by which the motion, or the change of figure, of a mass of matter is produced, we mean something more than simple pressure balanced by pressure, relatively at rest. In the latter case we have to consider only the pressures as they are balanced by each other, without any reference to motion. But in the former case no effect can be produced unless the pressure act through some portion of space.—If the pressure be increased in the same ratio that the space through which it acts is diminished, or *vice versa*, the same effect will still be produced. The space, therefore, compensates for the pressure, and the pressure for the space; and when taken together, they constitute a determinate measurable quantity of moving force, capable of producing effects of various kinds, but in determinate quantities which are always proportional to the moving forces by which they are produced.

The term *force* is often indiscriminately used to signify simple pressure, as well as to denote the compound quantity of force by which the motion of a body is produced.—The "force of gravity" for example, (mean-

ing quiescent pressure), and the "force of a body in motion," are very common expressions.—But these two descriptions of force are as different in kind, as lines are different from surfaces, or surfaces from solids; and they have been distinguished by various authors by different terms. From the following proposition it appears that Galileo applied the same meaning to *impetus* which was afterwards applied by Huygens to *ascensional force*. "Mobile grave descendendo acquirit eum *impetum*, qui illi ad eandem altitudinem reducendo sufficiat." *

Leibnitz and his followers adopted the distinctive terms, *vis mortua* and *vis viva*. Dr. Wollaston prefers *impetus* to *vis viva*, but he sometimes uses *energy* in the same sense. The Edinburgh reviewers approve of Dr. Wollaston's application of the term *impetus*; but they propose to apply the same meaning to *energy* which is applied by Sir Isaac Newton to *vis impressa*, namely the pressure multiplied into the time of its action.

Mr. Smeaton uses the term *mechanic power* to express the product of the pressure into the space through which it acts, or the product of the mass into the square of its velocity.

* Dialo. de Syst. Mund. p. 12.

In his definition of power (which I have quoted at page 129) he refers only to its effects in producing motion. But we have seen that he understands the same measure to be the proper one, whether the force be expended in producing motion or change of figure, and he concludes that the effects of force "cannot be so easily, distinctly, and fundamentally compared, as by having recourse to the common measure, viz. mechanic power." *

If this principle be capable of such general application, it is desirable that it should be denoted by a distinct term, in order to obviate ambiguity or misapprehension. The compound term *moving force* has been commonly applied, by various authors, to signify the action of moving pressure, as distinguished from quiescent pressure; and from its general use in this acceptation, I have been induced to adopt it.

It is sometimes indeed used for *motive force*, or the pressure uncombined with time or with the space through which it acts. But the two terms need not be confounded, and if *moving force* were defined to be "moving pressure producing change of velocity, or change of figure in masses of matter," it could not be

* Philos. Trans. 1776, p. 473.

easily misunderstood. For, if the *moving force* be estimated by the changes which it produces, the space through which the pressure acts, as well as the pressure, must be taken into the account. In the above definition it is necessary to adopt the expression "change of velocity" in preference to "change of motion;" because change of direction is included in change of motion; and change of direction cannot be estimated by the pressure combined with the space without reference to the time. The centripetal force which retains a body in a circular orbit, is similar to quiescent pressure;—the pressure at the centre moves through no space, and therefore there is no change of velocity; but if the revolving body approach or recede from the centre, any given space, the pressure moves through the same portion of space, and a corresponding change of velocity is produced. Excepting change of direction, however, the above definition and measure of *moving force* apply to every case of moving pressure of which we have any experience.

The *pressure* taken together with the *time* of its *direct* action, bears a constant relation to an important class of the phenomena of moving force producing motion in masses of matter. But when the pressure is applied

indirectly by levers, or other means, or when a change of figure is produced, the velocity of the pressure being different from that of the mass which is moved, this relation is no longer preserved. In cases of that description, the sum of the changes produced by the moving force, is not in any constant ratio to the time of its action. If this statement be correct, the relation between the effects of a moving force and the time of its action cannot be reduced to a general formula—It can only be considered as an individual character, or property of one class of the phenomena of moving force,—a property of great importance no doubt, but still not a general property. The *duration* therefore of a moving force cannot be taken generally as an element in the estimation of its quantity.

If we attempt to estimate some moving forces by their duration, and others by the spaces through which the pressure acts,—according to particular circumstances which may appear to be more favorable to the application of one measure than the other; we cannot avoid the inconsistency of sometimes concluding that a given quantity of moving force may be considered greater or less, according to the nature of the effect it is intended to produce.

This principle of moving force may perhaps be illustrated in some degree, by comparing the phenomena of force with those of heat.—Metals and fluids having been observed to expand and contract according as their temperature is increased or diminished, it was for a long time understood that temperature was the measure of heat. After it had been proved by Dr. Black that bodies of equal temperatures contain unequal quantities of heat, it was no longer contended that temperature could be taken generally as the measure of heat. Yet temperature is a most important property of heat, and in cases where the temperature and the heat increase and diminish in the same ratio, the temperature may be used as the measure of the heat.—In cases of moving force, where the *space* described by a constant pressure, and its *duration* increase in the same ratio, the duration may be taken as the measure of the moving force.—Of absolute motion or of absolute heat, we know little,—our researches are chiefly directed to relative heat and to relative motion.—In the estimation of deflecting forces, the duration becomes an important element.—In investigating the phenomena of liquefaction and evaporation, temperature becomes an essential consideration. Yet there appears to be no more

reason for taking duration as the general measure of moving force, than for taking temperature as the general measure of heat.

It has been shown (page 187) that if a given non-elastic body, moving with a given velocity, strike an equal non-elastic body at rest in free space, half the moving force of the striking body is expended in producing change of figure; and in the same manner it has been shown (page 197) that, when the mass of the striking body is half that of the body which is struck, two thirds of the moving force of the striking body is expended in producing change of figure.

Upon the same principles, the following general theorem is easily made out.—If any non-elastic mass A strike another non-elastic mass B at rest in free space, (the direction of the stroke passing through the centres of gravity of A and B,) the original moving force of A will be to that part of it which is expended in producing change of figure, as A+B : B, and to the remaining moving force of A and B after collision, as A+B : A. *

* The following is a demonstration of this. Let v = the velocity of A before collision; then $\frac{A\,v}{A+B}$ = the velocity of A and B after collision. The *moving force* before collision will be $A\,v^2$, and that after collision

The practical application of this principle is exemplified in a variety of instances.—In driving piles—if the weight of the ram be very small in proportion to that of the pile, a great part of its moving force is expended in bruising the pile, and the progress of the pile into the earth is very small. The heavier the ram is in proportion to the pile, the greater is the progress of the pile, by the application of the same quantity of moving force.—On the other hand, if the object be to produce a change of figure in the substance which is struck, in hammering iron for example, if the anvil be light in proportion to the hammer, the intended effect is not produced in the same degree as when the anvil, or the mass which is struck, is heavy in proportion to the hammer which strikes it. *

If a non-elastic body strike a non-elastic

$A+B.\left(\frac{Av}{A+B}\right)^2=\frac{A^2}{A+B}v^2$. But these two quantities are as $1:\frac{A}{A+B}$; hence it appears that the fractional part of the *moving force* found in the motion of the bodies after collision is $\frac{A}{A+B}$, consequently the part which is spent in producing change of figure is $\frac{B}{A+B}$.

* Examples of moving force similar to these are referred to by Mr. Leslie, in his excellent work on heat, p. 128. He explains them however on different principles.

machine moving with a uniform velocity (such as the float of an undershot water-wheel) the maximum effect of moving force will be communicated to the wheel when the part of it which is struck moves with half the velocity of the body which strikes it.

Let A (fig. 17) be a non-elastic soft mass, uniformly penetrable by the cylinder c, and moving in the direction AB with such a velocity v that it would be brought to rest by driving the cylinder up to F against an immoveable obstacle—If instead of an immoveable obstacle, we suppose B to be the float of a water-wheel moving with an uniform velocity $= \frac{1}{2} v$, and to be struck by c at F; in that case when B has moved through a space $FH = \frac{1}{2}$ EF, A will have arrived at G, EG being $= \frac{3}{4}$ EF, and will have lost half its velocity. In this operation $\frac{1}{4}$ of the moving force of A has been expended in changing the figure of A, $\frac{1}{4}$ remains with it when moving on with the same velocity as B, and the remaining $\frac{1}{2}$ has been expended in pressing B through the space FH, and it is easily demonstrable that if the velocity of B be either greater or less than $\frac{1}{2} v$, it will be pressed by c through a space less than FH. And whether A be uniformly penetrable by c or not, the same relative velocity of A and B is required

in order that the greatest possible quantity of the moving force of A shall be transferred to B.*—It would be too much to say that this explanation may be applied to the action of water on a water-wheel, but it is remarkable that these conclusions agree very nearly with the results of Mr. Smeaton's experiments. (See page 160).

The expenditure of moving force in overcoming the cohesion of the particles of fluids is always exhibited under very complicated

* To mathematical readers it may perhaps be acceptable to have the problem in a more general form.

Problem. Given two non-elastic bodies, A and B, such that A, moving with a given velocity, v, shall overtake B, moving with a variable velocity, x, in the same right line; it is required to find x, such that the increase of *moving force* found in the motion of B after the stroke may be a maximum.

Solution. Let $y =$ the velocity of B after the stroke. By mechanics, $\frac{Av+Bx}{A+B}=y$; and per question, $By^2-Bx^2=$ maximum. That is, $B.\overline{\frac{Av+Bx}{A+B}}^2-Bx^2=$ maximum. Reduced, $2Bvx-(A+2B)x^2=$ maximum.

In fluxions $2Bv\dot{x}-(A+2B)2x\dot{x}=o$, or $Bv=(A+2B)x$, & $x=\frac{B}{A+2B}v$. Q.E.I.

Cor. 1. If B be indefinitely greater than A, then its velocity after the stroke will be the same as before, & $x=\frac{1}{2}v$, which is the case in the text.

Cor. 2. If B=A, then $x=\frac{1}{3}v$.

Cor. 3. If A be indefinitely greater than B, then $x=o$.

circumstances; but the amount of it may in some instances be estimated with considerable exactness. When a jet of water issues from an orifice of a particular construction, it has very nearly the same velocity which a body would acquire in falling freely through a height equal to the depth of the orifice under the surface of the water.—In that case therefore, a very small part only of the moving force is expended in changing the figure of the water before it reaches the most contracted part of the orifice.—But if the orifice be constructed so that any separation of the particles of the water from each other takes place, although they may be brought together again and completely fill the most contracted part of the orifice, yet there is invariably a considerable loss of moving force. In other words, a portion of the moving force is expended in producing this separation of the particles of the water; and that portion may be estimated by deducting from the whole moving force which the water would acquire in falling freely through the height of the head, that portion of moving force which is found to remain with the water after it has issued.

The following important proposition relating to this subject, is laid down by Daniel Bernoulli in his Hydrodynamics, page 278.

If a jet of water I (fig. 18) issue from the side of a vessel A, with the velocity which a body would acquire in falling freely from the surface B to C, he says the *repulsion* of the water in the opposite direction to the jet will be equal to the weight of a column of water, of which the base is equal to the section of the contracted vein, and the height equal to 2 BC.

This question respecting the amount of what has been termed the "reaction of the effluent water," derives additional interest from the circumstance of its having particularly engaged the attention of Sir Isaac Newton, and from his having given a solution of the problem in the first edition of the "Principia," which he materially altered in the succeeding editions. In the first edition (book 2d, prop. 37) he infers, that the reaction is equal to the weight of a column of water of which the base is equal to the area of the orifice, and the height equal to that of the surface of the water above the orifice. In the succeeding editions, the subject is more fully discussed in the 36th prop. of the second book, where he infers (cor. 4.) that, when the area of the surface B is indefinitely large compared with that of the orifice, the reaction is, what it was afterwards in a different manner

demonstrated to be by D. Bernoulli. Sir Isaac Newton further observes, that he found, by admeasurement, the area of the orifice in a thin plate to be to that of the section of the contracted vein, at the point of its greatest contraction, in the ratio of $\sqrt{2}:1$ nearly. He takes the re-action, therefore, to be greater than what he understood it to be when he published the first edition, in the ratio of $\sqrt{2}:1$ nearly. He refers, however, more to experiment than to theory for a solution of this question; and many valuable experiments have since been made on effluent water; yet I cannot find that the results of any direct experiments have been published which go to determine the precise amount of this re-action.

Sir Isaac Newton suggested (Principia, first edit. p. 332) a method by which the reaction may be easily measured. If the vessel be suspended like a pendulum, he observes, it will recede from the perpendicular in the opposite direction to the jet.—I have made some experiments on a vessel suspended in that manner, and in order to ascertain the re-action as accurately as possible, I made use of a balance-beam furnished with a perpendicular arm of the same length as the horizontal arms, as represented at fig. 18. The scales were exactly balanced, and the end of the rod

D made just to touch the side of the vessel. —The orifice was then opened, and the water in the vessel was kept uniformly at the same height by a stream falling gently on the plate E. The scale F having been raised by the reaction of the jet, weights were put into it till it was brought exactly to the position in which it was before the orifice was opened. The diameter of the vessel was 7 inches, and the height B C exactly 3 feet. I tried orifices of various diameters from .35 to .7 of an inch. Their exact diameters were ascertained by a micrometer, and the time carefully observed in which 30 lbs. of water were discharged through each orifice.

When the orifice was made in a thin plate ($\frac{1}{10}$ of an inch in thickness), I found the reaction to be greater than Sir Isaac Newton's first conclusion, in the ratio of 1.14 to 1. There was some variation in the results of the experiments. The greatest reaction, however, was as 1.16 to 1, and the least as 1.09 to 1, which fall far short of Sir Isaac Newton's last inference. The velocity of the water at the orifice (ascertained by observing the time in which 30 lbs. were discharged) was less than that which a body would acquire in falling freely from B to C, in the ratio of .6 to 1.

I found no constant ratio to subsist between the diameter of the contracted vein and that of the orifice; and observing considerable opacity in the jet at the contracted vein, I concluded it to be divided into a number of different filaments, and I gave up all hopes of ascertaining the actual area of the section of the stream at that place by measuring its diameter. After repeated trials I found that when the water issued through a contracted hole, of the shape represented at G, the jet was quite transparent, and the reaction (taking the mean of 12 experiments with 4 different orifices) was less than the weight of a column of water of twice the height of the head and diameter of the smallest part of the hole, in the ratio of .865 to 1. The least reaction was as .85 to 1, and the greatest as .88 to 1. By measuring the quantity of water delivered in a given time, I found the velocity of the jet, at the smallest part of the orifice, to be less than that which a body would acquire in falling freely from B to C, in the ratio of .94 to 1. The highest ratio was as .95 to 1, and the lowest .89 to 1.*

* Although these experiments were made since this paper was read before the Society, I have taken the liberty to insert the results, because they afford a good illustration of the principle which I have endeavoured to support.

From these results it appears, that when the contracted vein is not opaque, and when its velocity is nearly equal to that which is due to the head, the reaction is nearly equal to what it was concluded to be by Sir Isaac Newton and M. D. Bernoulli; and the great apparent difference between Sir Isaac Newton's first and second conclusions arises from his having been misled by some experiments to which he alludes. He says—"Per experimenta vero constat, quod quantitas aquæ, quæ, per foramen circulare in fundo vasis factum, dato tempore effluit, ea sit, quæ cum velocitate prædicta," [viz. the velocity due to the head] "non per foramen illud, sed per foramen circulare, cujus diametrum est ad diametrum foraminis illius ut 21 ad 25, eodem tempore effluere debet."* We must presume, however, that he refers to experiments made by others; for if he had made them himself, he would, no doubt, have arrived at the same results which have since been so well established by various authors, and he would have stated the above ratio to be as 19.5 to 25 nearly.

But his demonstration of the reaction requires that the velocity at the contracted vein shall be equal to that which is due to the head.

* Principia, edit. 2. lib. 2. prop. 36.

Now that velocity cannot be determined by measuring the imperfectly contracted vein in cases of water spouting through a hole in a thin plate.

We may safely indeed infer, that, in such cases, the velocity is considerably less than what is due to the head. For, the jet being opaque, some moving force must be expended in separating the particles from each other, and the distance to which the jet from such an orifice is projected on a horizontal plane, confirms that inference. The demonstration, therefore, of the reaction can be properly applied to such cases only as those where the water, issuing through a tube properly contracted, acquires the velocity nearly which is due to the head, and in those cases the experimental results agree, as I have stated, remarkably well with the demonstration.

These results agree also with the explanations which have been given of *moving force*. If we suppose the velocity of the jet to be equal to that which is due to the head, and the vessel to move uniformly in the opposite direction C D with the same velocity; the water will be at rest as it issues.

Let a represent the area of the smallest section of the orifice. Then while the vessel has moved through a space $=2$ B C, a quantity

of water represented by $a \times 2BC$ has descended from B to C, and has been brought to rest. But the reaction is $= a \times 2BC$, and this multiplied by 2BC, the space through which it has acted, gives $a \times \overline{2BC}^2$ for the amount of the moving force produced, which is exactly the quantity of moving force necessary to raise the column $a \times 2$ BC to the height BC, and to project it with the velocity 2BC. For, a moving force $= a \times 2BC \times BC$ will raise that column from C to B, and an equal moving force will generate the velocity 2BC in the same column, therefore $2a \times 2BC \times BC = a \times \overline{2BC}^2$ is the whole moving force necessary to restore that column to the place and condition in which it was before it began to descend; and as no moving force has been expended in producing change of figure, that quantity of moving force must be found in the reaction of the water through the space which the vessel has moved while the water descended and was brought to rest.

Upon the same principle an easy and simple explanation may be given, I apprehend, of the action of the hydraulic machine called Barker's mill. Let AB (fig. 19) be the perpendicular tube, and BC the horizontal arm; let v express, in feet per second, the rotatory velocity of the arm at the orifice C, and let the

water be supposed to issue with the velocity due to the pressure. Put $g=16\frac{1}{12}$ feet.

If BC be a cylindrical tube, and if q represent the quantity of water it contains from B to C, the centrifugal pressure upon a section of the arm at C, will be $\frac{qv^2}{4g\,BC}$; and whatever the length BC may be, the diameter remaining the same, q being as BC, the centrifugal pressure at C will always be as v^2; and it will be equal to the pressure of a perpendicular column of water whose height in feet is $\frac{v^2}{4g}$. Then if h express in feet the height AB of the water in the vertical tube, $h+\frac{v^2}{4g}$ will be the whole pressure at C; and if a express in feet the area of the most contracted section of the orifice, $2a\left(h+\frac{v^2}{4g}\right)$ will express the reaction, which being multiplied by v, the space through which it acts in a second, gives $2av\left(h+\frac{v^2}{4g}\right)$ for the *total* moving force of the arm in a second. But a part of this moving force is expended in producing the rotatory motion of the water, and in raising it to the height $\frac{v^2}{4g}$. For, if we suppose a perpendicular tube CP to rise from the arm at C, the surface of the water in that tube

would stand at P, PR being $=\frac{v^2}{4g}$. Now if instead of letting the water escape at C, it be allowed to flow over the perpendicular tube at P, and fill another similar perpendicular tube adjoining it, and issue from an orifice at the bottom of that tube, the effect must be the same as if it issued at C, and a moving force must be expended at C, sufficient to generate the velocity v, in the water which passes, and also to raise it from R to P.

The pressure at C being equal to the weight of a column of water whose height is $h+\frac{v^2}{4g}$, (that is =AB+PR), the velocity with which the water issues will be $\sqrt{4g\left(h+\frac{v^2}{4g}\right)}$ or $\sqrt{4gh+v^2}$. Let V express that velocity, then $a\mathrm{V}$ will express the quantity of water which passes in a second; and $2a\mathrm{V}\frac{v^2}{4g}$ will express the moving force necessary to generate the velocity v, in that quantity of water, and to raise it from R to P. That quantity of moving force being deducted from the total moving force of the arm, leaves $2av\left(h+\frac{v^2}{4g}\right)-2a\mathrm{V}\frac{v^2}{4g}$ for the *effective* moving force of the arm in a second.

That this is the effective moving force, may be shown also in another manner, as follows:

The *absolute* velocity of the water after it has left the machine will be $V-v$, and $\frac{(V-v)^2}{4g}$ will be the head which would produce that velocity; which being multiplied by aV, the quantity of water delivered in a second, gives $aV\frac{(V-v)^2}{4g}$ for the moving force which remains with the water after it has left the machine.

If that be deducted from aVh, the whole moving force of the water, there will remain $aVh-aV\frac{(V-v)^2}{4g}$ for the *effective* moving force, which will be found to be equal to $2av\left(h+\frac{v^2}{4g}\right)-2aV\frac{v^2}{4g}$, the *effective* moving force stated above.

The theory of this machine has occasionally occupied the attention of many distinguished mathematicians, and M. Euler has given two elaborate treatises on its principles in the memoirs of the Berlin Academy for 1750, p. 311, and for 1751, p. 271. His demonstrations relating to this subject are very compli-

cated, and they do not appear to have been adopted by succeeding authors.

Mr. Waring, of America, has given quite a different theory, which has been approved of by several good writers on hydraulics. He concludes that the greatest effect will be produced when the velocity of the orifice is half that of the issuing water; and that this effect will be nearly the same as that of a well-constructed undershot water-wheel.*

The explanation which I have offered of the action of the water on this machine is different from any other that I have had an opportunity of consulting. I offer it, therefore, merely as an attempt to solve an intricate problem.

If it were possible for the water to issue with the velocity due to the pressure, it is obvious, if my explanation be right, that although a very large proportion of the moving force of the water may be communicated to the machine, moving with a moderate velocity, the maximum of effect can only be obtained by an infinite velocity. But when the water issues with a velocity which is less than what is due to the pressure, as must always be the

* American Philos. Trans. vol. 3. p. 191 and 192.

case in practice, the velocity at which the maximum of effect is produced, may be found as follows. It should first be ascertained by experiment how near the issuing velocity can be brought to that which is due to the pressure. From the experiments which I have made, I have been led to conclude that no greater issuing velocity can possibly be obtained from a machine of this kind than what is due to .8 of the pressure. If this conclusion be correct, it follows that, whatever may be the issuing velocity of the water, a moving force, equal to $\frac{1}{4}$ of the moving force which is necessary to generate that velocity in the water, when falling freely, is expended in producing change of figure; that is, in forcing the water through the tubes and through the orifice C; and if the velocity of the machine be such that PC=5AB, the issuing velocity will be equal to the velocity of the orifice, and the whole moving force of the water in descending from A to B will be expended in producing change of figure.

For, the head due to V, the issuing velocity, will in this case be PR, which is also the head due to v, the velocity of the orifice. We shall therefore have $V=v$; and if CP represent the total moving force necessary to raise the

water from C to P, CR=AB will represent that part of it which is expended in producing change of figure. The greatest velocity, therefore, that the orifice, when the machine meets with no resistance, can acquire, will be $\sqrt{4g\times 4h}$.

When the velocity of the orifice is less than that, V will be greater than v; and $V-v$, the absolute velocity of the water after it has left the machine, will be $\sqrt{.8\,(4gh+v^2)}-v$. The head or the moving force expended in producing that velocity will be $\frac{\left(\sqrt{.8\,(4gh+v^2)}-v\right)^2}{4g}$.

The moving force expended in producing change of figure will be $.2\left(h+\frac{v^2}{4g}\right)$

Now when the sum of these two quantities, or $\frac{\left(\sqrt{.8\,(4gh+v^2)}-v\right)^2}{4g}+.2\left(h+\frac{v^2}{4g}\right)$, is a minimum, we shall find $v=\sqrt{2gh(\sqrt{5}-1)}=6.3056\sqrt{h}$ for the velocity of the orifice when the machine produces a maximum of effect; and in that case the above sum becomes $=.4472h$.

We shall therefore have $h-.4472h=.5528h$ for the maximum of effect, supposing h to

represent the whole moving force of a given quantity of water descending from A to B. This effect is considerably greater than that which the same quantity of water would produce if applied to an undershot water-wheel, but less than that which it would produce if properly applied to an overshot water-wheel.

Respecting the maximum of effect produced by machines, I wish to observe, that in the actual construction of machines it is necessary to aim at a maximum quite different from that which is usually proposed in books on the theory of mechanics. This will perhaps be best explained by examining the simple case where a given weight P, (fig. 20) connected with another W, by a string passing over the pulley F, descends vertically and raises W, without friction, from the horizontal line AC along the inclined plane AB. If we make AB : BC : : 2W : P, W will be raised to B in the least time ;* and upon this principle, the maximum of effect in machines is usually demonstrated in theory. In practice, however, the object is not merely to raise W to B in the *least time*, but to raise it with the least expenditure of *moving force*. When

* If the ascent be made in the least *possible* time, W must ascend not along the plane AB, but along a concave surface AGB.

it is raised in the least time, P must descend through a space =AB, but when it is raised with the least moving force, P descends through a space $=\frac{1}{2}$AB only. For, if we make BD$=\frac{1}{2}$AB, and let W ascend along any concave surface DEB, of which BD is the chord, it will be raised to B by the descent of P through a space =BD, and it will be at rest when it arrives at B. This is so obvious, that it would be superfluous to give a demonstration of it. It appears then, that twice the quantity of moving force which is absolutely necessary to raise W to B, must be expended if it is to be raised by P in the least time. To determine the curve by which W will ascend from D to B in the least time, is an intricate problem, and I do not know that it has ever been solved; but a practical approximation to it in any particular case may be easily found. A well constructed steam-engine for raising water exhibits in every stroke a practical example of the same problem. At the commencement of the stroke, a very great pressure of steam is thrown upon the piston, and this pressure is gradually diminished, so that at the end of the stroke there is a considerable preponderance in the opposite direction. In consequence of this

regulated pressure of the steam, the motion of the machine resembles the uniform vibrations of a pendulum, and the moving force of the steam is applied to the greatest advantage.

By proceeding on the principle that when W is raised to B in the least time, the maximum of effect is produced, many erroneous conclusions have been drawn respecting the proper construction of machines. It is laid down for example, on this principle, that "In an overshot water-wheel, the machine will be in its greatest perfection, when the diameter of the wheel is two-thirds of the height of the water above the lowest point of the wheel."* But it is very well known that there would be lost, by that construction, nearly one-third of the moving force of the water, which is saved by making the wheel one-half larger in diameter, and by making its velocity much less than what is required by the above rule.

It should be borne in mind, that the mechanical effects produced by means of machines, consist, almost invariably, of changes of figure. Even when a given mass is raised with an uniform velocity to a given height, a change of figure only is produced. For, if the mass

* Gregory's Mechanics, vol. 1, p. 447.

were pressed to the earth by the elastic force of a spring instead of the force of gravity, we should not hesitate to say, that a mechanical change of figure is produced when it is raised. Changes of figure of this kind being easily estimated, the raising of a given weight to a given height, has long been adopted as a convenient common measure for almost every kind of moving force. If the rule, quoted above, for the construction of an overshot water-wheel, had been tried by this measure, its fallacy would have been apparent.

Dr. Wollaston has described a case of collision and change of figure, which has been understood to prove, that the force of a body in motion may be properly estimated either by the duration of its action, or by the space through which it acts, according to the particular views which may be taken of the phenomena. C (fig. 21) is supposed to be a ball of clay, or any other soft and wholly inelastic substance, suspended at rest, but free to move in any direction with the slightest impulse; the two pegs, O and P, to be similar and equal in every respect, and to meet with uniform and equal resistance in penetrating C; the weight of A to be double that of B, the velocity of A moving in the direction AC, to be half that of B, moving in the opposite

direction BC, and A and B to strike their respective pegs at the same instant. The result will be as follows. C will remain unmoved, A and B will be brought to rest in the same time, and the peg P will be found to have penetrated C twice as far as it has been penetrated by O. This case appears to me to admit of the same explanation as some of those which we have already examined. It is considered by many, however, to show distinctly, that the forces of A and B are equal. If we confine our attention solely to the circumstance of C remaining at rest, we must no doubt conclude, that the opposite forces of A and B are equal; but if we attend to all the results of the experiment, we cannot consistently, draw that conclusion. It has often been asserted by the advocates on both sides of this question, that we can judge of forces only by their effects; yet it has been contended by M. D'Alembert,* and by many other good writers on dynamics, that the estimation of forces by their total effects, involves a metaphysical question which ought not to be mixed with experimental investigations of physical facts. It may be safely affirmed, however, that nothing can be more strictly grounded upon

* Traité de Dynamique, Disc. Prélim. p. 22.

experiment, than conclusions derived from the examination of mechanical changes of figure.

This term, as has been already observed, includes every change of figure which requires moving force, or pressure acting through some portion of space, to produce it. Whether it be the repulsion or the cohesion of the integrant parts of bodies, or the attraction of masses to each other, that is to be overcome, mechanical change of figure is produced; and we have seen, in various cases which have been examined, the uniform relation which subsists between determinable quantities of change of figure and the moving forces by which they are produced. We find by experience, that when a body in motion is retarded or brought to rest, either a change of figure is produced, or a quantity of moving force, equal to that which the body has parted with, is communicated to some other body or system of bodies. It has been supposed, indeed, that A and B, in the case stated, may be brought to rest without any change of figure being produced. That supposition, however, is contradicted by universal experience, and in point of fact we may, with as much consistency, suppose that a body may be put in motion without force, as that two bodies moving in opposite directions may destroy each other's motion

without producing change of figure. It appears then, that if any metaphysical consideration has been improperly mixed with this question, it is the supposed possible existence of perfectly hard non-elastic substances. But unless we have actual proof of the existence of such substances, we can have no evidence derived from experience to justify the inference, that A and B may be brought to rest without producing change of figure. When a physical experiment of any kind is made, it is generally understood, that unless all the results be collected and examined, erroneous conclusions may be formed. If an experimenter reject some of the results which he obtains, on the supposition, that sometimes they may not occur, although in fact they constantly occur in determinate quantities, he cannot reasonably demand assent to general conclusions drawn from so partial an examination of the facts. If this reasoning be well founded, we cannot reject the consideration of the changes of figure produced by A and B; and if we have no experience of a mechanical change of figure being produced without moving force, nor of bodies destroying each other's motion without producing mechanical change of figure, we cannot, in the case before us con-

sistently do otherwise than estimate the absolute forces of A and B by the respective changes of figure produced by each.

I shall now conclude my observations with a simple application of the principle which I have endeavoured to support, to the resolution of compound moving forces.

If we suppose BAC (fig. 22) to be a right angle, and three strings, AB, AC, and AE, in the same plane, to be united at A; the strings AB and AC to be prolonged to a length indefinitely great, when compared with the diagram, and the end of each of the three strings to pass over a vertical pulley. If the parallelogram be completed, and if three weights *m*, *n*, and *o*, which are to each other as AD, AB, and AC respectively, be suspended by the respective strings AE, AB, and AC, they will balance each other, and the strings will coincide in direction with the diagonal and sides of the parallelogram. If the weights be set in motion, by taking from *m* an indefinitely small part of its weight, *n* and *o* will descend, raising *m*, and the point of junction of the strings will move in the direction AD. When that point has arrived at D, the weight *m* will have ascended a space equal to AD, *n* will have descended a space equal to AB, and *o* will have descended a space equal to AC.

The quantity of moving force therefore, is, on one side $m.AD$, balanced on the other side by $n.AB+o.AC$; the moving force of each string being as the weight suspended to it multiplied into the space through which it has moved. So that in this case, where the parallelogram is right angled, the moving forces in the different directions are as the squares of the diagonal and the respective sides of the parallelogram.

When BAC is not a right angle, let the parallelogram be completed, and the weights suspended as before, and draw DF and DG (fig. 23) perpendiculars to AB and AC. If the weights be set in motion, the point of junction of the strings will move in the direction AD, and when that point has arrived at D, the weights m, n, and o, will have moved through the spaces AD, AF, and AG respectively. The moving force, therefore, is on one side $m.AD$ balanced by $n.AF+o.AG$ on the other side; or the moving forces in the different directions are respectively as the square of AD, the rectangle AB. AF, and the rectangle AC. AG.

This conclusion, however, involves the geometrical proposition, that the square of AD is equal to the sum of the rectangles AB. AF and AC. AG, a property of the triangle which

is demonstrated in the first prop. of the fourth book of Pappus; and that prop. unfolds, as he observes, a general principle, including the properties demonstrated in the I. 47, and VI. 31, of Euclid. For the following concise demonstration, I am indebted to my friend Dr. Roget. Draw BH and CI perpendiculars to AD. Then the triangles ABH and ADF being similar, AB : AD :: AH : AF. Also ACI and ADG being similar, AC : AD :: AI(=HD) : AG. From these proportions we obtain the following equations $AB.AF=AD.AH$ and $AC.AG=AD.HD$, which being added together, give $AB.AF+AC.AG=AD.AH+AD.HD=AD.(AH+HD)=AD^2$.*

Various other interesting and useful examples might be given of the application of the measure of moving force, which consists of the pressure multiplied into the space through which it acts; but I believe I have already exceeded the proper limits of a dissertation of this kind, and doubtful as I must be of the favourable reception of the reasoning which I have adopted, I am more disposed to curtail than to lengthen it.

By way of recapitulation, however, I wish briefly to observe, that we appear to derive all

* The same proposition is demonstrated in the II. 19. of Professor Leslie's Elements of Geometry.

our notions of force from pressure as it is perceived by the sense of touch, and that in all cases where neither the velocity nor the figure of the body pressed is changed by the pressure, we have only simple pressure balanced by pressure, the various combinations of which have long ago been explained and demonstrated in the most satisfactory manner.

But in all cases where either the velocity or the figure of the body pressed is changed by the pressure, we have examples of moving force, which may be properly represented by a rectangle; of which the pressure forms one side, and the space, through which it acts, the other side: and however various and complicated the changes of velocity and of figure may appear, they must all be derived from determinate quantities of moving force. We may have changes of rectilineal velocity in various directions, changes of rotatory velocity, and changes of figure, all produced at the same time by a given quantity of moving force; and it is certainly a desirable object to determine what portion of that quantity has been expended in producing each of these different effects. I have endeavoured to show that all these changes may be distinctly explained and estimated, by examining the pressure and the space through which it acts in producing them.

In objecting to the opinions of many eminent writers on mechanics, I have ventured much. Although this has not been done inconsiderately, I am sensible there are in the arrangement of my arguments some faults, and others which have escaped my observation, will no doubt occur to the reader. But if my endeavours to make this essay more free from imperfections than it is, had been successful, it would still be unreasonable to expect it to obtain more attention than has been paid to the arguments of the illustrious men who have preceded me in the same track of investigation. If I have succeeded so far only as to show, that the prevailing doctrines of force, especially in their application to practical purposes, involve some difficulties which are unexplained; and if I have offered any inducement to men of science to reexamine this question, my chief object will in a great measure be accomplished.

Errata.

Page	Line	
116	27	for "E and C," read "P and Q"
121	21	for "and when," read "and, if FH and FI be taken each $=\frac{1}{4}$EF, when"
123	15	after "right angles" insert "and if AC=AB"
150	17	for "effect, &c." read "effect of a given quantity of water must consequently be as c^2"
176	12	for "force acting at," read "pressure acting through a small space at"
176	14	for "DH will be," read "and if DH represent"
215	8	of the Note, for "theory" read "theoretical measure of force."

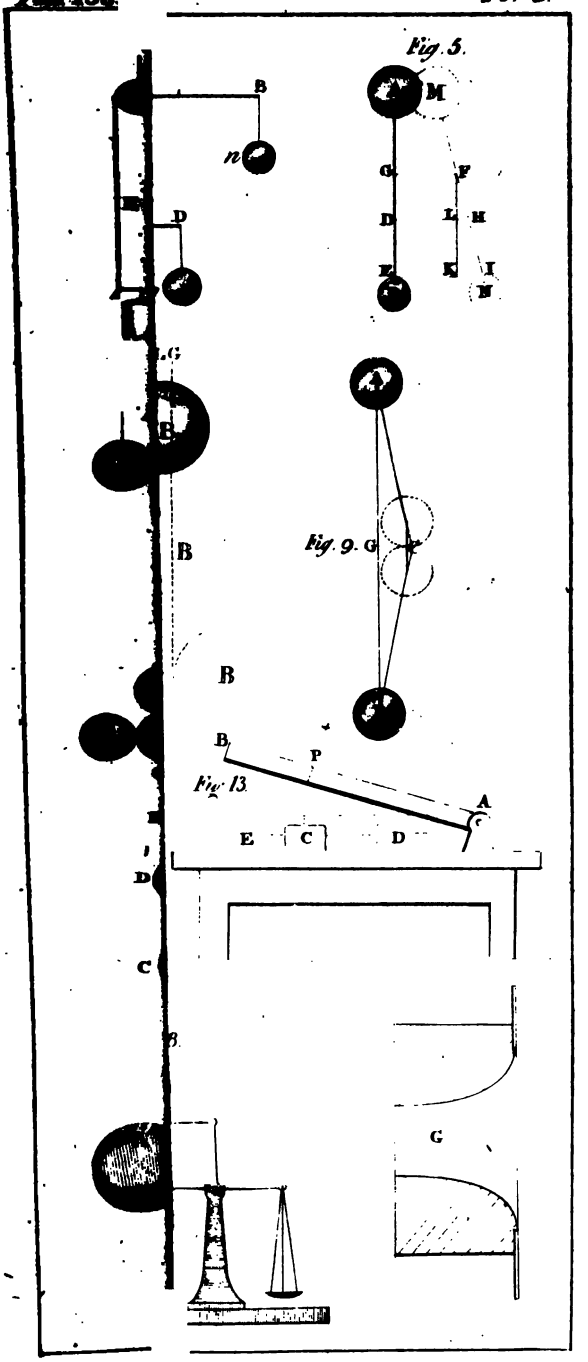
Fig. 5.
B
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Fig. 9. G
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Fig. 13.
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9.

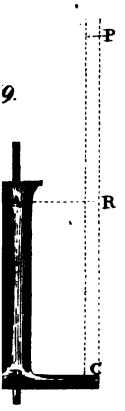


Fig. 20.

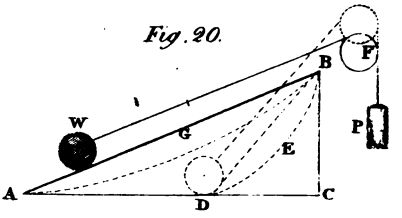


Fig. 21.

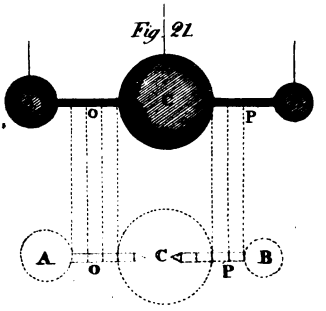


Fig. 22.

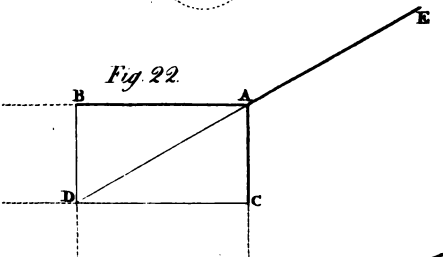
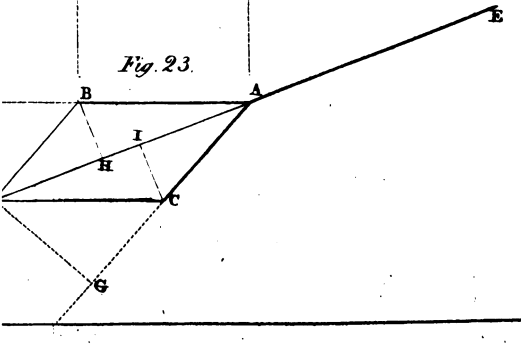


Fig. 23.



Account of a remarkable EFFECT *produced by* A STROKE OF LIGHTNING;

In a Letter addressed to Thomas Henry, Esq. F.R.S. &c. President of the Literary and Philosophical Society, from Matthew Nicholson, Esq. - With Remarks on the same, by Mr. Henry.

(Read October 20, 1809.)

Liverpool, 12th Sept. 1809.

Dear Sir,

I have complied with your request in the best way I could, by soliciting and receiving Mr. Chadwick's assistance in the description of the Thunder-storm which happened at his house; and which description I mean herein to inclose—leaving it to you, Sir, to make what use of it you think proper.

I am, dear Sir,

Your obliged humble servant,

MATTHEW NICHOLSON.

Some Facts respecting a Thunder-storm at the house of Mr. Elias Chadwick, of Swinton, in the parish of Eccles, and county of Lancaster, on Sunday the 6th of August, 1809—*reported from information communicated upon the spot, and since extended and revised, by Mr. Chadwick himself—4th September,* 1809.

MR. CHADWICK'S house is situated five miles from Manchester, between the roads leading from thence to Wigan and to Bolton, upon the elevated range of country which lies to the right of the river Irwell. The near prospects from the house are towards the South-west, over the Worsley coal-district, and the flat country bounded by the rivers Irwell and Mersey below Manchester. A considerable part of this scene consists of a bog called Chat-moss. If the house itself be not over coal, it is probably owing to some derangement of the strata, for coal is procured at different depths through the neighbourhood in almost every direction.

The plan (plate 3, fig.2) may give some idea of the buildings, where BCDE is the scite of a coal-vault with its open entrance at F, and of a

water-cistern over it. The building for these purposes was made of bricks, with a lime-cement, which holds water. Its foundation, and the bottom of the vault, were about one foot below ground. The walls, three feet thick, were about twelve feet high, strengthened by bond-timbers. The top and bottom of the cistern, and all its walls, were covered with large flags. The whole was about eighteen feet long, eight feet broad, and eleven feet high above ground; and there was in the vault at the time about one ton of coal.

About half past twelve at noon, after repeated peals of distant and approaching thunder in the lower country, the heavens became suddenly enveloped in thick darkness; and it was thought prudent to open all the windows and doors of the house, as the best preparation for receiving the expected storm. No sooner was this done, than a tremendous explosion occurred; the effect of which was the removal of the outside wall of the described cistern, from its upright position shewn by the sketch G, (fig. 3) into the inclined position, intended to be represented in the sketches H and K, as it now stands, with its coping entire, among the shattered fragments of the end-walls. It may be necessary, as the sketches are not perfectly correct, to say, that

the end of the outside-wall next the entrance to the vault was removed about nine feet, the other end only about four feet.

Mr. and Mrs. Chadwick were standing in the passage L. Mr. Chadwick was suddenly turned half round; but neither of them were injured. A young man of seventeen years old, also received the shock unhurt, and was the first to communicate the astonishing event, which had occurred out of doors; for he alone, standing in the stable about twenty-four feet distant, saw the cistern wall remove from its place, which it did, not instantaneously, but gradually. Two young trees at twelve feet distance appear untouched. The bond-timbers of the cistern were forced by the shock to a greater distance than the brick-work, and were apparently scorched. That part of the building, which was removed and is yet standing, contains about seven thousand bricks. The wall seems to have been lifted from its foundations. The weight of the works, removed and thrown down, is probably not over-rated at twenty-six tons, inclusive of the flags and mortar. Some water was in the cistern, but the quantity is unknown. No metals, excepting slender spout-brackets, were near the place, and these were not even disturbed. A leaden pipe for conveying the

water into the adjoining kitchens, had also sustained no injury. Immediately after the explosion, rain fell in a torrent, deluging for a moment every thing around; and for a few minutes the air in the nearer parts of the house was offensively smoky and sulphureous.

Such were some of the circumstances and effects attending an event, in itself awful; and perhaps, unequalled in the records of this part of the world.

REMARKS

On the Foregoing Narrative,

BY MR. HENRY.

THE very extraordinary circumstances, attending the storm described by MR. NICHOLSON, called to my recollection an account of a thunder-storm near Coldstream, in Scotland, related by Mr. Brydone, in a letter to Sir Joseph Banks, which is inserted in the 77th volume of the Philosophical Transactions. Of the leading facts, detailed in that communication, the following is a brief abstract.

The storm of thunder and lightning, alluded to, happened on the 19th of July, 1785. In

the early part of it, the interval of time, between the flashes and the arrival of the sound, was so considerable, as to allay all apprehensions of danger in Mr. Brydone and his family, who were watching the progress of the tempest. Suddenly, however, they were alarmed by a loud report, for which they were not prepared by any immediately preceding flash. It resembled the firing of several muskets, rapidly succeeding each other, and was not followed by a rumbling noise like the other claps. After this, the clouds began to disperse without any subsequent disturbance.

At this moment, and at a small distance from the place where Mr. Brydone and his companions were observing the tempest, James Lauder, who had just crossed the Tweed, sitting on the fore-part of his cart, and had nearly gained the summit of an ascent about 70 feet above the bed of the river, was suddenly killed by an electric discharge, together with the two horses which he was driving. Part of the iron work of the wheels was found, on examination, to be in a state of incipient fusion, and the wood connected with it was shattered and dispersed; but though the heat had affected the metal thus strongly, there were no marks of combustion on the timber. About four feet and a half behind

each wheel of the cart, was a circular hole in the ground, about 20 inches diameter. The earth and small stones seemed as if they had been torn up by the violent strokes of a pick-axe, and were thrown on each side of the road. On pushing back the cart, in the same track it had described, to the spot where the accident had happened, the marks of fusion on the wheels were found to correspond with the centres of the holes in the ground. Yet another cart, which was following at the distance of about 24 yards lower down the hill, was not injured; the driver, though he had his companion full in view and was stunned by the report, perceived no flash; nor was he aware of any unusual sensation.

From the above and other circumstances, it appears probable that the electric fluid, which occasioned the disaster, did not proceed directly from an impending cloud, but was discharged from the earth. The manner in which this might happen, has been explained by an ingenious theory of EARL STANHOPE,* of which the following is a summary outline.

Let ABC (plate 3, fig. 1) represent a cloud of several miles in length, one end only of which approaches the earth within striking distance at G. Let another cloud DEF be

* Philos. Trans. vol. 77.

imagined to extend beneath the former, and a portion of it at E to be nearly within striking distance of the road at LM, where the two carts may be supposed to have been passing. Both clouds may be assumed to be positively electrified. When the upper cloud discharges itself violently into the earth at G, the electricity of the lower cloud, hitherto condensed by the contiguity of the upper one, will rush at DA to restore the equilibrium in the latter. The electricity of the earth at LM, which had hitherto remained quiescent, though condensed by the electrical atmosphere of the lower cloud, being now freed from the superincumbent elastic pressure, will issue, with great force, into the contiguous cloud DEF, destroying or greatly injuring the imperfect conductors through which it passes. This mode of action of the electric fluid, Earl Stanhope has denominated the *returning stroke*. "It accounts," his Lordship has observed, "for the loud report of thunder that was unaccompanied by lightning at L or at M. The report must be loud from its being near; but no lightning could be perceived at L or M by reason of the thick thunder cloud DEF being situated immediately between the spectator at M and DA, the place between the two clouds where the lightning was."

A sketch of the supposed Position of the Clouds &c. in the storm described by Mr Brydone, from the Philosophical Transactions.

Page. 266. Plate. 3.

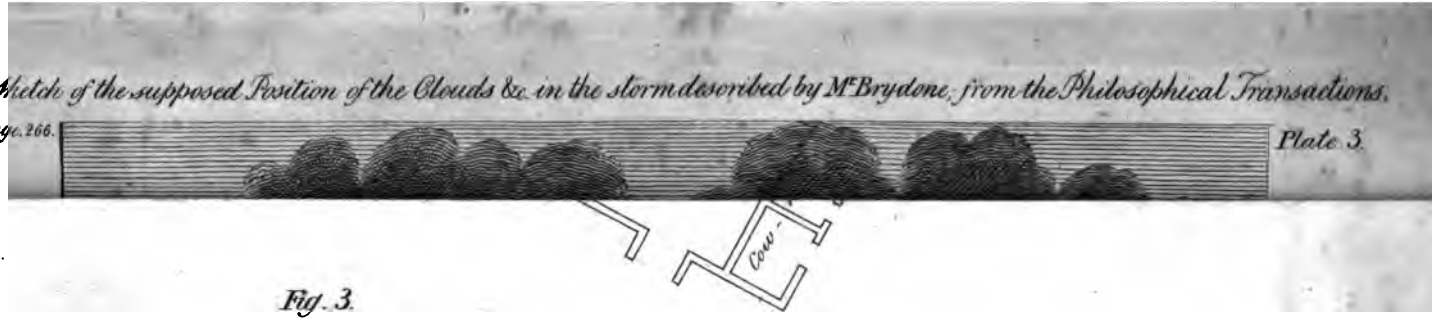
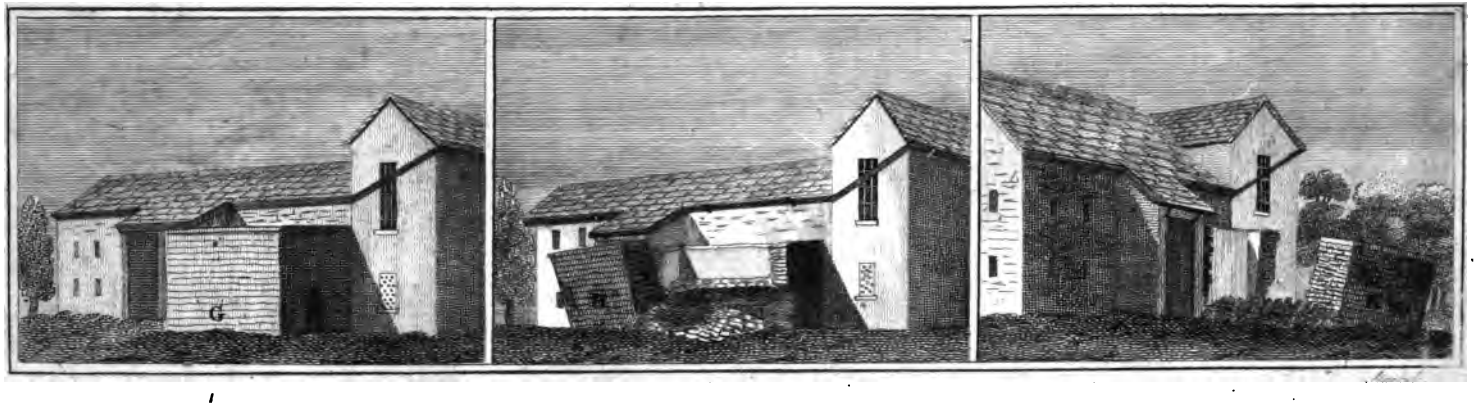


Fig. 3.



The foregoing narrative and ingenious theory may tend to explain, in some degree, the extraordinary event at Swinton. In both instances, the thunder and lightning, which were observed previously to the great explosions, were distant. In the Scotch storm, though distant lightning had been visible, no flash was perceived at the place where Lauder, the driver of the cart, was killed; nor does it appear that any flash attended the destructive explosion at Swinton. The great darkness at the latter place renders it probable that, according to the hypothesis, there were distinct clouds at different altitudes. It can scarcely be doubted that the electrical current passed from the earth to Lauder's cart; nor can we imagine that such a mass of brick and stone work, as formed the cistern at Mr. Chadwick's, could have been lifted and moved from its foundation either by a *main* or *lateral* stroke. A proof, indeed, that it was not, is, that the wall was left upright with its coping entire. Mr. Chadwick, who was standing in the house, was turned half round, which motion was probably caused by the action of the electric fluid on his feet. In the same storm, by which Lauder was destroyed, shocks were felt in several places in the vicinity, but were not immediately preceded by lightning. A

little before the fatal accident, a tremulous motion of the earth was perceived by a respectable witness; and a man in a hayfield, who was thrown down, complained of having received a violent blow on the soles of his feet. In one respect, the circumstances at the two places were dissimilar. No rain succeeded the explosion in Scotland, where there had been a long continued drought; but the storm at Swinton was followed by a very heavy shower; and the four days immediately preceding the storm had been showery.

From a meteorological journal, kept by Mr. Hanson, house-surgeon of the Manchester Lying-in Hospital, it appears that there were four remarkable changes in the pressure of the atmosphere, from the second to the eleventh of August. On the evening of the sixth, there was much distant thunder and lightning. On that day, the barometrical column was much augmented, and indicated the greatest variation in the space of twenty-four hours. The range of the thermometer, on the same day, was from 68.5° to 55°; and the wind, from the 4th to the 8th inclusive, was west. On the 10th we had one of the most tremendous thunder storms ever experienced in this part of the country; and, during the whole month, the most violent and fatal tempests raged in almost every part of the kingdom.

The quantity of rain, that fell at Manchester during the month of August, 1809, amounted, according to Mr. Hanson's journal, to 3.875 inches; and at Malton in Yorkshire, by Mr. Stockton's register, it reached the enormous quantity of 9.7 inches. At Bingley, in the West-Riding of that county, it is stated at 4.96 inches. It would add greatly to the value of meteorological registers, if they were made to include the variations in the electrical state of the atmosphere; and from a comparison of these changes with the tables of diseases, kept by medical practitioners in the same situations, it is not improbable that valuable inferences might be drawn, especially respecting the cause of the prevailing epidemics.

Connected with the present subject, it may be remarked, that the temperature of the atmosphere during the last summer, though the season was distinguished by numerous and awful storms, has been unusually low. On the 10th of August, the day so remarkable for the violence and permanence of the thunder storm, it did not exceed 68°, and the mean heat of the day was only 55°.88.

THEOREMS AND PROBLEMS,

INTENDED

To elucidate the mechanical principle called

VIS VIVA.

BY MR. JOHN GOUGH.

(Communicated by Dr. HOLME.)

DEFINITIONS.

1. *Force* is an abstract term comprising motive force, retarding force, resistance, and the *vis viva* of mathematicians on the continent.

2. *Motive force* is any force, that increases the motion of a body on which it acts.

3. *Retarding force* is any force that diminishes the motion of a body subjected to its influence.

4. *Resistance* is a force constantly exerted by a body, when affected by pressure or percussion, to preserve its figure unchanged.

5. *Quantity of resistance* is the whole force thus exerted by a body, while its figure is undergoing a certain change.

6. *Vis viva* is the whole force opposed by a body in motion, to a retarding force which impedes its progress: and, conversely, it is the

whole force accumulated in a body by the action of any motive force, which puts that body into motion.

Axioms, *or Maxims derived from universal Experience.*

1. Forces are magnitudes, consequently two forces of the same kind have a ratio the one to the other.

2. If two bodies, which are equal and alike in all respects, move with equal velocities; their *vires vivæ* are also equal.

3. A motive or retarding force is equal to a force, which, by acting in a contrary direction, would preserve the body, thus acted on, in a state of uniform motion or rest.

4. The *vis viva* of a body is equal to the quantity of resistance, which it is able to overcome.

THEOREMS.

Theorem I. If two bodies, A and B, move with equal velocities, their *vires vivæ* will be directly as their masses or quantities of matter; that is, put F = *vis viva* of A; f = *vis viva* of B; a = mass of A; b = mass of B; and we have as $F : f :: a : b$.

For let t be a mass which measures a and b, and let g be its *vis viva* when it moves with

the velocity common to A and B. Now F and f are magnitudes by *ax.* 1st: therefore they may be divided into equal parts, as well as the masses a and b to which they belong: but a and b have been divided into masses, each of which is equal to t; and each of these masses moves with the velocity common to A and B; therefore g denotes the *vis viva* of each of them by axiom 2d. Hence it follows that a and F are equimultiples of t and g, a and F being divided into an equal number of parts; for the same reason b and f are equimultiples of the same magnitudes; consequently as $F : f :: a : b$ by Euclid V. 4. Q. E. D.

COROLLARY 1. If two bodies be acted on, for the same or equal intervals of time, by motive or retarding forces, which are as their masses; the *vires vivæ* acquired or lost by them are also as their masses, or as the *momenta* acquired or lost by them. For the accelerative forces are equal in this case by dynamics, and the times being equal, the velocities are equal; therefore as $F : f :: a : b$ by the proposition: But when the velocities are equal, the *momenta* are as the masses: Hence as $F : f :: M : m$.

COR. 2. Bodies, which ascend or descend for equal times near the surface of the earth,

acquire or lose quantities of the *vis viva*, which are as the *momenta* acquired or lost by them in equal intervals of time, because the motive force of gravity is as the matter on which it acts.

THEOREM II. Suppose two mediums, whose powers of resistance, P and p, act uniformly, to be penetrated by any mechanical means whatever, to the depths S and s: Put R and r for the quantities of resistance surmounted in penetrating them; and we have as $R : r :: PS : ps$.

For since P and p are magnitudes by ax. 1, they may be represented by right lines: Assume the right line AC, (plate 4, fig. 1) in which take CB, making AC : CB :: $P : p$; also make CE perpendicular to AC; in which take CT=S and CV=s; also let CE measure both CT and CV: complete the parallelograms CTAa, CVBb, and draw EG parallel to AC, meeting Aa in G, and Bb in H: lastly divide the lines CV, CT, by means of the common measure CE into the equal parts CE, EI, IV, VL and LT.

In the first place, let $P=p$; then CA=CB; let n= quantity of resistance required to penetrate the medium having the resistance p, from C to E, by def. 5; and the same quantity will be again demanded to carry the work

on from E to 1, as well as from I to V: Hence r and CV are equimultiples of n and CE; therefore as $n : r :: CE : CV ::$ rectangle CH: rectangle BV: let $q =$ quantity of resistance which would penetrate the same medium through the space CT; and we shall have for the same reason, as $n : q ::$ rectangle CH: rectangle BT.——Now let P be greater than p; and AC will be greater than BC.

In this case, the force required to penetrate the medium whose resistance $= p$, through the space CE, will be to that required to penetrate that whose resistance $= P$, through the same space, as BC to CA, by def. 5.; that is, the quantities of resistance of the two mediums will be in the same ratio, by axiom 4. Hence by equimultiples $q : R ::$ rectangle BT: rectangle Ca; but as $q : r ::$ rectangle BT: rectangle Cb; consequently as $R : r ::$ rectangle Ca: rectangle Cb:: $PS : ps$. Q.E.D.

Cor. 1. When P is constant, R has a constant ratio to PS; but when p is variable, $\dot{r}$ has a constant ratio to $p\dot{s}$, by prime and ultimate ratios; hence as $R : \dot{r} :: PS : p\dot{s}$.

Cor. 2. As $F : f :: R : r$, by axiom 4; hence as $F : f :: PS : ps$, P and p being invariable; but if p be variable, we have as $F : f :: PS : p\dot{s}$; that is, F has a constant ratio to PS;

and this is true whether P is a motive or retarding force, by def. 6.

Cor. 3. If gravity be the motive force, and a, b, the masses acted on, it will be as $F:f::aS:bs$; for it is as $P:p::a:b$ in this case.

Cor. 4. If the velocities of the bodies be equal, we shall have by theor. 1. as $F:f::a:b$; hence by cor. 2. as $a:b::PS:ps$, and p equal $bPS\div as$; therefore if gravity be the motive force acting on the body whose mass $=a$, we have $P=a$; and $p=bS\div s$, or the weight of a body which is equal to the resistance opposed to the given body whose mass $=b$, by the medium, which it penetrates; this follows from the 3d axiom.

Theorem III. Put u and v for the velocities of the bodies a and b, and we have as $F:f::au^2:bv^2$ universally.

Case 1. Let P and p be constant forces, and it will be by cor. 2. theor. 2. as $F:f::PS:ps$; but as $PS:ps::au^2:bv^2$, Emerson's Mechanics, prop. 6; hence as $F:f::au^2:bv^2$.

Case 2. Let one of the forces P and p be variable, namely, p; then by cor. 2. theor. 2. as $F:f::PS:ps$; but ps is in constant proportion to bvv, by Emerson's Fluxions, (sect. 3, prob. 2, 1st edit.); therefore the fluent, or f, is in constant proportion to bv^2; moreover

PS or F is in constant proportion to au^2. Mechanics, prop. 6; hence as $F:f::au^2:bv^2$. Q.E.D.

Cor. 1. As $PS:ps::au^2:bv^2$.

Cor. 2. Let gravity be the motive force acting on a, then $P=a$; also put $c=16\frac{1}{12}$ feet, and we have $u=32\frac{1}{6}$ feet$=2c$, and it will be, by cor. 1, as $ac:ps::4ac^2:bv^2$; hence $4cps=bv^2$.

Cor. 3. If $P=p$, we have $F:f::S:s::u:v$. For $F:f::PS:ps::S:s::au^2:bv^2$; but $au=bv$, Mechanics, prop. 4th; therefore as $F:f::S:s::u:v::b:a$; and this corollary is true when the force P or p is variable; for in this case it will be as $\dot{F}:\dot{f}::\dot{S}:\dot{s}::a\,u\,\dot{u}:b\,v\,\dot{v}$.

Cor. 4. If m be the *momentum* of b, f will be in constant proportion to $\frac{m^2}{b}$ or to mv; hence if b be one of a system of bodies in motion, its *vis viva* will be affirmative in all cases; because m^2 is affirmative, and the signs of m and v are always alike.

Theorem IV. If a, b, d, &c. be the masses of any number of bodies moving with the velocities u, V, v, &c. which they have acquired by the uniform action of the motive forces P, p, q, &c. in passing through the spaces S, s, t, &c.; we have $4c\times(PS+ps+qt$, &c.$)=au^2+bV^2+dv^2$, &c.

For $4cPS=au^2$; $4cps=bV^2$; $4cqt=dv^2$, by cor. 2. theor. 3.; hence, by addition, $4c\times(PS+ps+qt$, &c.$)=au^2+bV^2+dv^2$, &c. Q.E.D.

Cor. 1. If g be the sum of the *vires vivæ* of the bodies, whose masses are a, b, d, &c. g will have a constant proportion to $au^2+bV^2+dv^2$, &c.

Cor. 2. Let $n=a+b+d$, &c. $x=$ its velocity when g denotes its *vis viva*, and we have by cor. 1. $nx^2=au^2+bV^2+dv^2$, &c.; hence $x=\sqrt{\frac{au^2+bV^2+dv^2}{n}}$.

Scholium. It appears from the last corollary, that a system of bodies in motion has an assignable quantity of *vis viva*, even when the *momentum* of it, or the motion of its centre of gravity is equal to nothing.

Theorem V. Let P, Q, R, &c. (plate 4, fig. 2) be a system of bodies in motion; whose common centre of gravity is G, moving in absolute space, in the direction Gg: put $y=$ the velocity of G in Gg; $u=$ the velocity, with which P approaches to, or recedes from G in the relative space P, Q, R; V the same kind of velocity in respect of Q, and v in respect of R; and let a, b, d, denote the masses of P, Q, and R: then the *vis viva* of the system will be as $(a+b+d).y^2+au^2+bV^2+dv^2$.

For each of the bodies P, Q, R, moves in absolute space, namely, in the direction Gg, with the velocity y, common to them all; consequently the sum of their *vires vivæ* in this direction is as $(a+b+d)\,y^2$, by cor. 1. theor. 4. But the *vires vivæ* of P, Q, R, in the relative space PQR are respectively as au^2, bV^2 and dv^2 by theor. 3; hence the sum total of these forces is as $(a+b+d).\,y^2+au^2+bV^2+dv^2$, cor. 1. theor. 4. Q.E.D.

Cor. 1. If P, Q, and R be at rest in the relative space PQR, they move only in absolute space with the velocity y; that is the *vis viva* of the system is equal to that of its centre of gravity; because $u=V=v=o$; and the figure of the system undergoes no change; because P, Q, and R preserve their relative positions unaltered.

Cor. 2. But if u, V, and v be real quantities, the *vis viva* of the system exceeds that of its centre of gravity, by the theorem. For the same reason, the bodies P, Q, and R are not at rest in the relative space QRP; that is the figure of the system is undergoing a change; consequently if P, Q, and R react upon each other from any cause whatever, the foregoing excess of *vis viva* will be exerted to overcome this reaction; which will continue

until a quantity of resistance has been surmounted equivalent to the excess in question, by axiom 4.; at which time the figure of the system will become permanent by the last corollary; if no new disturbing force intervene.

COR. 3. The excess of *vis viva*, pointed out above, is exerted altogether in the relative space PQR; consequently the mutual reaction of the parts of a system can not alter the *vis viva* of its centre of gravity; therefore the same cause does not change the *momentum* of this point by cor. 4. theor. 3; which agrees perfectly with the common dynamics.

SCHOLIUM. In estimating the change of figure, produced in a system by the reaction of its parts, we may consider the centre of gravity to be at rest, and take notice only of the velocities of the constituent parts relative to the centre of gravity; in which case we shall have, $a\,u+b\,V+d\,v=o$.

THEOREM VI. Let APE, BQE, (plate 4, fig. 3) be two bodies, which meet in E; put k = their relative velocity, and a, b for the masses of APE, BQE respectively; and the quantity of *vis viva* exerted on the system APQB, to change its figure, will be as $\frac{a\,b\,k^2}{a+b}$.

For let G be the centre of gravity of the

system; put u and v for the absolute velocities of APE and BQE respectively; then the *vis viva* of the system is as au^2+bv^2 by theor. 4; but the velocity of G$=\frac{au+bv}{a+b}$, by dynamics, where the sign, connecting the terms of the numerator, is affirmative when the bodies APE, BQE move in the same direction, and negative when they move in contrary directions; now the *vis viva* of G is as $\frac{a^2u^2 \pm 2abuv+b^2v^2}{a+b}$, by theor. 3; but the excess of au^2+bv^2 compared with this expression is as the *vis viva* which acts on the figure of the system by cor. 2. theor. 5; which excess $=ab\times\left(\frac{u^2 \pm 2uv+v^2}{a+b}\right)$ now when the bodies move in the same direction $u-v=k$ and when they move in contrary directions $u+v=k$; therefore the excess in question $=\frac{abk^2}{a+b}$. Q. E. D.

Cor. 1. The *momenta* of the bodies APE, BQE are equal, in the relative space APQB; because they add nothing to the *momentum* of the centre of gravity G; therefore the *vis viva* of APE before concussion is to the *vis viva* of BQE, as the velocity of the former, to that of the latter, or inversely as their masses, by cor. 4. theor. 3: moreover the velocities of A P E, B Q E are respectively equal to $\frac{bk}{a+b}$ and $\frac{ak}{a+b}$.

Cor. 2. Let P and Q be the centres of gravity of the bodies, when they come into contact at E; p and q their centres of gravity, when they are at rest in the space PABQ by Cor. 2, Theor. 5; then as action and reaction are equal at E, GE will be constant in all cases; because G and the space PABQ move with equal velocities; and from the nature of the centre of gravity it will be as PG : pg :: QG : qg. Now let the bodies APE, BQE be homogeneous and pliant; and it is evident, that one of the points P or Q moves faster than G; from whence it follows that pg and gq are less than PG and GQ; i. e. the points P and Q approach each other and the common centre of gravity G, while the mutual reaction of the bodies is exerted to reduce the *vis viva* of the system to that of its centre of gravity. Let us suppose in the next place APE to be an indefinitely hard body, and BQE to be soft; then APE will suffer no change of figure; PG (or PE+EG) will be constant; that is, pg=PG; therefore qg=QG and pq=PQ, in which case the body BQE undergoes all the change. Lastly, if both the bodies be infinitely hard, neither will suffer any change; and PQ will remain invariable: but a force situated in G will act equally on their respective centres of gravity in the

directions GP and GQ thereby giving to each body equal quantities of motion in opposite directions. In this imaginary case then, the whole force of APE and BQE acts in the character of momentum; consequently the *vires vivæ* of bodies arise from the soft and pliant texture of all substances with which men are acquainted. This observation affords a clear distinction of *momentum* and *vis viva*: the former is a force, which one body exerts on another to change its motion in absolute space; but the latter is employed in overcoming the continued reaction of resisting mediums, and in altering the figures of soft and elastic bodies.

Cor. 3. Let P, Q, R, &c. (plate 4, fig. 4.) be the centres of gravity of three or more bodies situated in the right line PR; in which some or all of them move so as to bring all of them into contact; moreover let a, b, d, &c. be the masses of P, Q, R, &c; k the relative velocity of P and Q; l that of P and R; n that of Q and R; t = the mass of the system $= a+b+d$, &c: I say the quantity of *vis viva* exerted on the system to change its figure, is as the sum of the rectangles of each pair of bodies drawn into the square of their relative velocity directly, and inversely as the mass t; or it is as $\frac{abk^2}{t}+\frac{adl^2}{t}+\frac{bdn^2}{t}$. For let u, V, v,

&c, denote the absolute velocities of P, Q, R, &c, and proceed as in the demonstration of the theorem.

THEOREM VII. Let APE and BQE, (plate 4, fig. 3), be two homogeneous and elastic bodies in motion, which meet at E; put F and f for the quantities of *vis viva* which are exerted during the stroke, on APE and BQE, to change their figures, and let their masses be denoted by a and b; and it will be as $F : f :: b : a$; that is, these quantities of *vis viva* will be inversely as the quantities of matter on which they act.

For, since the bodies APE and BQE are homogeneous, their powers of resistance are equal; therefore the point E, and their common centre of gravity G, are at rest in the relative space APQB, by cor. 2. theor. 6; and the motion of APE, as well as that of BQE, is opposed by a force acting at G in the contrary directions GP and GQ, by mechanics, prop. 44. cor. 4; hence we have as P p : Q q :: $b : a$, by cor. 3. theor. 3; but as $F : f ::$ P p : Q q, by cor. 2. theor 2; because the resisting powers are equal in both directions; therefore as $F : f :: b : a$. Q.E.D.

COR. 1. The *vis viva*, exerted on an elastic body, does not become inactive by changing the figure of that body. On the contrary

the force remains accumulated in the matter, constantly ready to restore its original shape; that is, the *vis viva* infused into a body of this description acts on the cohesion of its constituent particles. Let g= the *vis viva* thus infused; d= the mass, which receives it; t= the intensity of its action or its effect on a given part of d; and t is as $\frac{g}{d}$; and g as $t\,d$.

Cor. 2. We have, by the theorem, as $F : f : : b : a$; but by cor. 1. as $F : f : : a\,T : b\,t$; hence as $T : t : : b^2 : a^2$; that is, the intensity of the *vis viva* accumulated, by collision, in A P E, is to the same power in B Q E; as the square of the mass of B Q E, to the square of the mass of A P E, hence it happens, that when two homogeneous elastic bodies of very different magnitudes strike each other, the less is broken while the greater remains uninjured: because the smaller body receives the greater quantity of *vis viva* in a less portion of matter.

Cor. 3. The theorem is equally applicable to homogeneous bodies which are soft and ductile; now by the demonstration of the theorem, as $F : f : :$ P p : Q q; that is, the *vis viva* exerted to change the figure of a body is as the space through which its centre of gravity is compelled to move by that effort.

MECHANICAL PROBLEMS;

The solutions of which depend for the most part on the preceding theorems.

Problem I. If a cube, whose height =1 inch, and weight =1 pound, move with the velocity acquired by falling freely through 193 inches, and strike with one of its faces an indefinite mass of soft matter, which it penetrates to the depth of 7·72 inches before its velocity is destroyed: required a weight which is equal to the resistance of the matter to the face of the cube?

Solution. We have in cor. 4, theor. 2. $a=1=b$; $S=193$; $s=7{\cdot}72=\frac{193}{25}$; hence p or the required weight, equal $\frac{bS}{s}=\frac{S}{s}=25$lbs. Q.E.I.

Problem II. If a sphere, and cube equal to its circumscribing cube, move with equal velocities, and fall upon an indefinite bed of matter, having an uniform resistance, in such a manner that the cube strikes the bed with one of its faces, and sinks n inches perpendicularly into it: what is the perpendicular depth to which the sphere will sink; supposing the bodies to be homogeneous, and neglecting the action of gravity?

SOLUTION. Put d= the diameter of the sphere and height of the cube; j=3·14159, &c; also let F, f, R and r denote the quantities, which they represent in theor. 2. Then the masses of the cube and sphere are as d^3 to $\frac{j d^3}{6}$, or as 6 to j; therefore as $F : f :: 6 : j$, by theor. 1; but as $F : f :: R : r$, by ax. 4. hence as $6 : j :: R : r$; but R is as $n d^2$, the matter removed by the cube; therefore as $6 : j :: n d^2 : \frac{j n d^2}{6}$ = the matter removed by the sphere. Now if $\frac{j n d^2}{6}$ be less than $\frac{j d^3}{12}$ or half the sphere, the depression made by the globe is a segment less than a hemisphere, the perpendicular height of which is $\frac{2n}{d}$; but if $\frac{j n d^2}{6}$ be greater than $\frac{j d^3}{12}$ that is, if n be greater than $\frac{d}{2}$; the depression made by the globe is a cylindrical pit, having a hemispherical bottom, the perpendicular height of which is $\frac{4n+d}{6}$. Q E.I.

PROBLEM III. Let AOB and aob, (plate 4, fig. 5.) be two levers, revolving with the angular velocities C and c about the points O and o; and let two material points, whose masses are B and b revolve with the levers; these things being supposed, let two forces F and f act for an instant at the points A

and a, so as to disturb the angular velocities of B and b, denoted by C and c; it is required to find, by the doctrine of the *vis viva*, what ratio the fluxion of C has to the fluxion of c?

SOLUTION. Let B and b move through the arcs BE and be, with the absolute velocities u and v, while the forces F and f are acting at the points A and a; let P and p be two forces which would produce the same changes of motion in the material points B and b, by ing at the distances OB and ob, which are produced by F and f at the distances OA and oa; put A=OA; D=OB; a=oa; d=ob. By the laws of circular motion, we have as $D^2 . C^2 : d^2 . c^2 :: u^2 : v^2$; and by fluxions, as $D^2 . C\dot{C} : d^2 . c\dot{c} :: u\dot{u} : v\dot{v}$; but by theor. 3. as $P . BE : p . be :: B . D^2 C\dot{C} : b . d^2 c\dot{c}$; now as $BE : be :: D . C : d . c$; hence as $P . D : p . d :: B . D^2\dot{C} : b . d^2\dot{c}$; but as $B . D : b . d :: A . F : a . f$; therefore as $\dot{C} : \dot{c} :: \frac{AG}{BD^2} : \frac{af}{bd^2}$. Q.E.I.

COR. 1. If F and f be constant forces acting at A and a on B and b, it will be as $C : c :: \dot{C} : \dot{c} :: \frac{AF}{BD^2} : \frac{af}{bd^2}$: also, if F and f be variable, but have the constant ratio of N to n, it will be as $C : c :: \frac{AN}{BD^2} : \frac{an}{bd^2}$.

COR. 2. If $C=c$, in the prob. or $\dot{C}=\dot{c}$ in cor. 1, we shall have, as $AF:af::BD^2:bd^2$.

COR. 3. Hence if $AF=af$, then $BD^2=bd^2$; that is, the *vires vivæ.* of B and b are equal, and $B:b::d^2:D^2::v^2:u^2$.

COR. 4. Consequently when $AF=af$, if B and b be so placed, as to receive equal angular velocities from the forces F and f; they also acquire equal quantities of *vis viva* at the same time.

PROB. IV. Let it be required to find the centre of gyration of a system of material particles b, l, k, (plate 4, fig. 5,) revolving about a given point o, in consequence of a force f, acting at a, perpendicular to the arm a o, of the compound lever o b k l a?

SOLUTION. Assume O as a centre of rotation; and let O B represent the radius of gyration to the system $b\,k\,l$; make A O=a o; and let the force $F=f$, act at A, perpendicular to A O; then F. A O is in constant proportion to O B^2. $(b+k+l)$, by cor. 1, prob. 3, and the definition of the centre of gyration. Now f, acting at a is, divided into as many parts as there are particles b, k and l; let p, q and r, be these parts; p. a o, q. a o and r. ao, are as $b\times b\,o^2$, $k\times k\,o^2$ and $b\times l\,o^2$, by cor. 2, prob. 3; therefore as p. a o : b. $b\,o^2$ :: f. a o : b. b o^2+k. $k\,o^2+l$. $l\,o^2$; but p. a o:

$b \cdot b\,o^2 :: F \cdot AO : OB^2 \cdot (b+k+l)$, *ibid*; hence $OB^2 \cdot (b+k+l) = b \cdot b\,o^2 + k \cdot k\,o^2 + l \cdot l\,o^2$, and $BO = \sqrt{\left(\frac{b \cdot b\,o^2 + k \cdot k\,o^2 + l \cdot l\,o^2}{b+k+l}\right)}$. Q.E.I.

Cor. 1. The centre of gyration of a system b, k, l, is also the centre of its *vis viva*; that is, if a material point, B, whose mass $= b+k+l$, &c. revolve round the centre O at the distance O B with the angular velocity of the system b, k, l, the *vis viva* of B is equal to the *vires vivæ* of the particles, b, k, l, &c. by cor. 4, prob. 3, or theor. 4.

Cor. 2. If o, the centre of rotation, coincide with the centre of gravity of b, k, l, the system has no momentum, (mechanics, prop. 50); but it has a quantity of *vis viva* equal to that of B, by the last corollary; hence if the parts of a system move amongst themselves, it has a quantity of *vis viva* by this cor. and theor. 4, whatever may be the state of the centre of gravity.

Cor. 3. Let G be the centre of gravity of the system b, k, l; join o G, in which produced, take o R = O B, the radius of gyration to the point o; also make G r = the radius of gyration to the point G; put o R = R, o G = g, G r = r, then $g^2 + r^2 = R^2$, by mechanics; but the system revolves with equal angular velocities about the points o and G; therefore the absolute velocity of R may be resolved into the

absolute velocities of G and r, consequently the *vis viva* of the point R may be resolved into the *vires vivæ* of the points G and r; because the quantities of matter, supposed to move with these three points, are equal.

PROBLEM V. Required the centre of oscillation of the system *b*, *l* and *k*?

SOLUTION. Let O S (plate 4, fig. 5,) be the length of a simple pendulum, which vibrates through similar arcs in equal times with the system *b*, *k* and *l*, vibrating upon the point o; and let the matter in the point S be equal to all the matter in *b*, *k* and *l*; make o s=O S; and s is the centre of oscillation required. Now to find the length of O S or o s, we are to consider that the matter in the system acts by its weight at G perpendicular to the horizon to give the point R a certain angular velocity; and the matter in the pendulum acts at S in the same direction to give G S the same angular velocity; therefore put O S=s; and we have as $g : s :: R^2 : s^2$ by cors. 1 and 2, prob. 3; hence as $g : R :: R : s$. Q.E.I.

COR. 1. $R^2=gs$.

COR. 2. If the system *b*, *k*, and *l*, revolve about the point o; put t= the time of revolution, m= the matter in *b*, *k* and *l*: and the *vis viva* of the system is as $\frac{m R^2}{t^2}$, or as $\frac{m g s}{t^2}$;

for, the velocity of the point R or the centre of gyration, is as $\frac{R}{t}$; therefore the *vis viva* of the same centre is as $\frac{m R^2}{t^2}$, by theor. 3; therefore the *vis viva* of the system is as $\frac{m R^2}{t^2}$, cor. 1, prob. 4, or as $\frac{m g s}{t^2}$, by the last corollary.

PROBLEM VI. Let there be two cylinders A and B of the same ductile matter whose diameters are a and b, and heights c and d, respectively; and let these cylinders be drawn out in length until their diameters become $= \frac{a}{m}$, and $\frac{b}{n}$; what is the ratio of the forces F and f, required to produce these changes?

SOLUTION. When the cylinders have been drawn as directed in the problem, the length of A $= m^2 c$; length of B $= n^2 d$; and the heights of their centres of gravity above the plane, on which they stand, are as their lengths, or as $m^2 c$ to $n^2 d$; but the heights of the centres of gravity of A and B above the same plane were as c to d in their first shape; therefore the spaces through which their centres of gravity move, while their figures are changing, are as $(m^2-1) \cdot c$ to $(n^2-1) \cdot d$; consequently as $F : f :: (m^2-1) \cdot c : (n^2-1) \cdot d$, by cor. 3, theor. 7; where the diameters a, and b, are not found in the proportion. Q.E.I.

EXAMPLE. Let A and B be two wires, the first 5, and the latter 3 inches long; and let A be drawn to one tenth, and B to one fourth of its original diameter; and we have $F : f :: 99 \times 5 : 15 \times 3 :: 11 : 1$.

PROBLEM VII. If a brittle ball A be broken by falling with the velocity u, on a larger ball B of the same matter; with what velocity v, must B strike another ball C larger than itself, to be broken in like manner?

SOLUTION. Put a, b and c = the masses of A, B and C; then $\frac{a\,b\,u^2}{a+b}$ = the *vis viva* exerted to change the figure of the system A and B, by theor. 6; and the quantity employed to change the figure of it is as $\frac{a\,b^2\,u^2}{(a+b)^2}$ by cor. 1, theor. 6, and theor. 3; for the same reason, the force employed to change the figure of B when it falls upon C, is as $\frac{b\,c^2\,v^2}{(b+c)^2}$: now the intensities of these forces in A and B must be equal; because they produce equal effects; but the intensity is as the *vis viva* directly and mass inversely, by cor. 1, theor. 7; therefore $\frac{b^2\,u^2}{(a+b)^2} = \frac{c^2\,v^2}{(b+c)^2}$; hence as $c \,.\, (a+b) : b \,.\, (b+c) :: u : v$. Q.E.I.

COR. If C be indefinitely great, it will be as $a+b : b :: u : v$.

Fig. 1

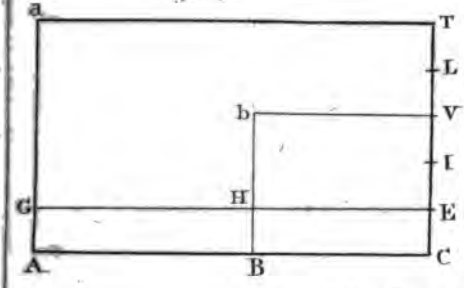


Fig. 2

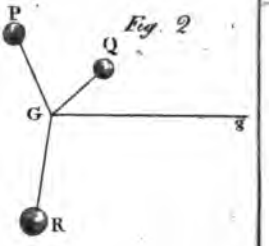


Fig. 3

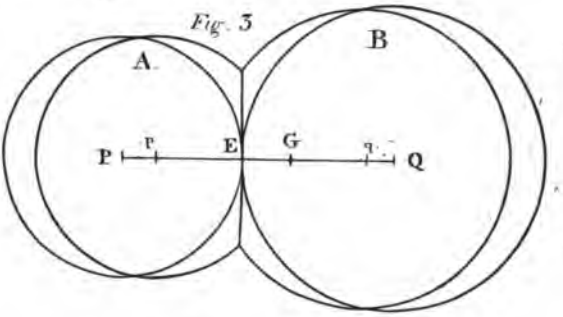


Fig 4

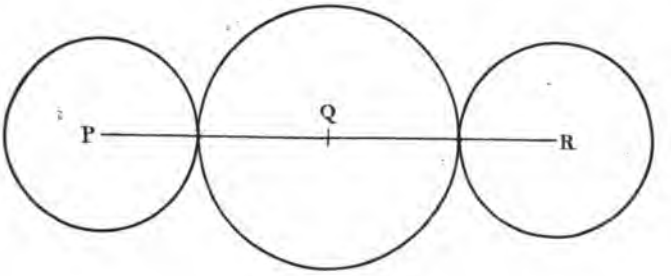
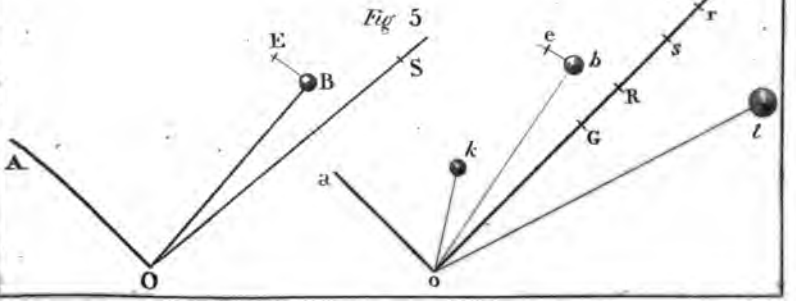


Fig 5



ON THE

THEORIES

OF THE

EXCITEMENT

OF

GALVANIC ELECTRICITY;

BY

WILLIAM HENRY, M.D.F.R.S, &c.

SEVERAL theories have been framed to account for the origin of the electricity, which is excited by the Galvanic pile, and by similar arrangements. Of these, the first in the order of time was proposed by the distinguished philosopher* to whom we are indebted for some of the earliest, and therefore the most difficult, steps in this department of science. The hypothesis was suggested by a fact, which may be considered, indeed, as fundamental to it. It had been observed by Mr. Bennett, so long ago as the year 1788, and afterwards confirmed by Volta himself, that electricity is excited by the simple apposition of different kinds of metals. The best way of exhibiting this fact is to take two discs or plates, the one of copper, the other of zinc;

* Signor Volta, in Nicholson's Journal, 8vo. i. 135.

to apply them to each other, for an instant, by their flat faces, and afterward, separating them dexterously, to bring them into contact with the electrometer. The instrument indicates, by the divergence of its gold leaves, what kind of electricity each of the plates has acquired; which proves to be positive in the zinc plate, and negative in the copper one.

To explain the phenomena, in the experiment which has been just described, it has been supposed by Volta, that, during the contact of the plates, a movement of the electric fluid takes place from one plate to the other; and that the zinc acquires just as much as the copper has lost. The metals, therefore, he denominates *motors of electricity*, and the process itself *electromotion*, the latter of which terms has been adopted by Mr. Davy. From subsequent experiments, Volta ascertained that the metals stand to each other, in this respect, in the following order; it being understood that the first gives up electricity to the second; the second to the third; the third to the fourth; and so on:

Silver,
Copper,
Iron,
Tin,
Lead,
Zinc.

It is to this transference of electricity, that Volta ascribes the whole of the phenomena, exhibited by Galvanic combinations. According to his view, the interposed fluids act entirely by their power of conducting electricity, and not at all by any chemical property. The effect of a series of Galvanic plates, or of a Galvanic pile, he believes to be nothing more than the sum total of the effects of several similar couples or pairs. Why the evolved electricity is determined to one end of the series, and exists there in its greatest force, I shall attempt to explain by the following illustrations.

If a plate of zinc be brought into contact, on both sides, with a plate of copper, it may be considered as acted upon, in opposite directions, by equal forces, which destroy each other. No alteration, therefore, takes place in its state of electricity; nor does any change happen, even when we substitute, for one of the copper plates, a third metal; on account of the trifling difference between the electromotive powers of bodies of this class. But liquids, possessing this power in only a very small degree, may be brought into contact with one of the zinc surfaces, without impairing the electromotive effect; and acting merely as conductors, they convey the excited electri-

city from the zinc plate, across the contiguous cell, to the next copper plate.

Let us imagine, then, a series of copper and zinc plates, arranged in pairs for any number of repetitions; (See the Diagram in plate 5, fig. 1,) with cells between each pair for the purpose of containing a fluid. Before these cells are filled, every copper plate will, according to the hypothesis, be in the state of negative, and every zinc plate in that of positive electricity. Let us farther suppose the natural quantity of electricity in each copper and zinc plate, before they are brought into apposition, to be denoted by q, and that, when the electricity has passed from the copper to the zinc, the ratio of the quantities in each may be as $1 : m$.* Let now the cells be filled with a conducting fluid; every pair of contiguous plates of copper and zinc will still maintain their relative proportions of electricity, viz. as $1 : m$. But, by reason of the conducting power of the fluid, the electricities of the first zinc and second copper plates will be equalized; as, in succession, will be also those of the zinc plate 2, and copper plate 3, &c. Now in order to find the relative quan-

* For the algebraical expression of this theory, which, in the paper as originally read, I had stated in common numbers, I am indebted to my friend Mr. Dalton.

tities of electricity in the several pairs of plates, when an equilibrium in the arrangement is effected, if n equal the number of pairs of plates, then $2nq$ = the total quantity of electricity in all of them taken together. Let x = the quantity of electricity in the first copper plate of the series; then, by hypothesis, mx = that of the contiguous or first zinc plate; also mx = the quantity in the second copper plate (by reason of the conducting fluid); but $1 : m :: mx : m^2x$ = the quantity in the second zinc plate. In like manner the quantities in the successive copper and zinc plates may be found, and will constitute this series;

	1	2	3	4		n
Copper plates,	x,	mx,	m^2x,	m^3x,	&c.....	mx^{n-1}
Zinc plates,	mx,	m^2x,	m^3x,	m^4x,	&c.....	mx^n

Hence it appears that the quantities of electricity in the successive plates of copper or of zinc form a *geometrical* progression, the ratio of which is m. Also the total quantities of electricity in the successive pairs of plates form a series in *geometrical* progression, as under.

Pairs of pl.	1	2	3	4	
Quant. of El.	$\frac{1}{1+m.x}$	$m.\frac{1}{1+m.x}$	$m^2.\frac{1}{1+m.x}$	$m^3.\frac{1}{1+m.x}$	&c.

From the above theory of Galvanic action it necessarily follows, that if the effect of a pile be in proportion to the *difference* in the

electricities of the first and last plates of the series, a pile of 50 pairs will not be exactly half so energetic as one of 100 pairs, but somewhat less; because the differences in the terms of a geometrical series increase as the terms increase. But, in the present instance, there is great reason to apprehend that the ratio of 1 to m is very nearly that of equality. If so, the geometrical series for a moderate number of terms, will scarcely differ from an arithmetical one. This accords very nearly with experience; for it has been determined by Volta, that if a combination of 20 pairs of plates produce a given effect on the electrometer, a series of 40 will produce double the effect; one of 60 triple, and so on. At the same time it is probable that the electric intensity of the plates, composing each pair, relatively to one another, continues unaltered, notwithstanding the change in their absolute quantities of electricity.

When a connection is established between the two extremities of a series like the above, for example between the third zinc plate, or its contiguous cell, and the first copper plate, the opposite electricities tend to an equilibrium. The third pair loses a share of its electricity, which is gained by the first; and the intermediate pair, being placed between opposite forces

of perhaps equal amount, remains *in equilibrio*. Hence, in every Galvanic arrangement, there must be a pair of plates at or near the centre in the natural state of electricity. A communication, between the two extremities of a pile would therefore reduce it to a state of permanent inaction, if there did not still exist some cause, capable of disturbing the equilibrium. On the hypothesis of Volta, this can be nothing else than the property of electromotion in the metallic plates, which has been described as the primary cause of all the phenomena.

This theory, on first view, appears sufficiently to explain the facts on electrical principles, without the interference of chemical action. Consistently with the hypothesis, different fluids, when made parts of Voltaic arrangements, produce effects more or less energetic, as they are more or less active in conducting electricity; the only property, according to Volta, that can be considered as influencing their efficiency in the pile. There are several facts, however, which, if not absolutely irreconcileable with the hypothesis, are certainly not at all explained by it. Why, for instance, it may be asked, when pure water forms a part of the arrangement, is the

action of the pile suspended by placing it in an exhausted receiver, or in any of those gases that are incapable of supporting oxidation? Why is its efficiency increased by an atmosphere of oxygen gas, or by adding, to the water in the cells, several fluids, in a proportion not sufficient to change materially its conducting power? Why is the nitric acid, though a worse conductor of electricity than the sulphuric, more active in promoting the energy of the apparatus? Why is the power of these combinations proportional to the disposition of one of the metals composing them to be oxidized by the interposed fluid? These facts undoubtedly suggest that, in some way or other, the chemical agency of the fluids employed is essential to the sustained activity of the pile. The principle has even been conceded by some distinguished electricians, who have attempted to explain it in different ways.

To account for the effect of the interposed fluids, Mr. Cuthbertson has suggested a theory, which is both ingenious and sufficiently feasible.* With Volta, he assumes the electromotive change in the metals to be the first in the order of phenomena. And when (he observes) the copper has given, and the

* Nicholson's Journal 8vo. ii. 287.

zinc has received, all the electricity, which their mutual powers require, if any menstruum be presented, which is capable of effecting a change in the metallic property of the two bodies, a change in their electrical states must, at the same time, happen. But as the alteration of metallic property is only superficial, the change of electrical condition will, also, be only at the surface; and the interior part of the zinc plate, retaining its property of resistance, the electric fluid, evolved at its surface, will necessarily be propelled forwards, through the menstruum, to the next copper plate of the series. This, however, can only happen in a progressive manner, because the fluid is but an imperfect conductor, a condition indispensible to the maintenance of any Galvanic intensity.

The explanation of Mr. Cuthbertson is unquestionably a valuable supplement to the theory of Volta, in as much as it takes into account the efficiency of chemical menstrua. These, consistently with his view, will evolve electricity the more freely, in proportion as they destroy more rapidly the metallic property of the plates of zinc. The hypothesis, however, is defective, because it fails to account for some of the phenomena;—why, for example, the action of the menstruum is

chiefly, if not entirely, exerted in oxidizing and dissolving the zinc plates; and why the evolution of hydrogen gas, or of nitrous gas, occurs chiefly at the copper surfaces.

An hypothesis, originally suggested by Fabroni, and reversing those which have been already stated, has been adopted by several eminent philosophers in our own country. It assumes the oxidation of the metals composing galvanic arrangements to be the *cause*, and not the *effect*, of the evolution of electricity. In the solution of a metal (it has been observed by Dr. Wollaston) * it would appear that electricity is evolved by the action of the acid upon the metal; and, in cases where hydrogen is disengaged, that this evolution is required to convert the hydrogen into gas. When a piece of zinc and another of silver are immersed in very dilute sulphuric acid, the zinc is dissolved and yields hydrogen gas; the silver, having no power of decomposing water, is not acted upon. But as soon as the two metals, placed under the diluted acid, are made to touch, hydrogen gas arises also from the surface of the silver. In this case, it is added, we have no reason to suppose that the contact of the silver imparts any new power; but merely that it serves as a conductor of

* Phil. Trans.

electricity, and thereby occasions the formation of hydrogen gas.

The chemical theory of the Galvanic pile, though already suggested in general terms, may be considered however, as having been a mere outline, till Dr. Bostock undertook to give it greater distinctness and consistency.* To the extended hypothesis, which he has proposed, it is necessary to admit, as a ground work, the three following postulates; 1stly, that the electric fluid is always liberated or generated, when a metal or other oxidizable substance unites with oxygen; 2dly, that the electric fluid has a strong attraction for hydrogen; and 3dly, that when the electric fluid, in passing along a chain of conductors, leaves an oxidizable substance, to be conveyed through water, it combines with hydrogen, from which it is again disengaged when it returns to the oxidizable conductor.

To the efficiency of the pile, two circumstances, it is observed by Dr. Bostock, are essential; that the electric fluid be disengaged; and that it be confined and carried forward in one direction, so as to be concentrated at the end of the apparatus. The first object is fulfilled by the oxidizement of the zinc; the second, Dr. Bostock supposes, is effected by

* Nicholson's Journal 8vo. iii. 9.

the union of the evolved electricity with nascent hydrogen, and by the attraction of the next copper plate for electricity. At the surface of this plate, the hydrogen and electricity are supposed to separate; the hydrogen to be disengaged in the state of gas, and the electricity to be conveyed onwards to the next zinc plate. Here, being in some degree accumulated, it is extricated in larger quantity, and in a more concentrated form, than before. By a repetition of the same train of operations, the electric fluid continues to accumulate in each successive pair; until, by a sufficient extension of the arrangement, it may be made to exist at the zinc end of the pile in any assignable degree of force.

The hypothesis of Dr. Bostock agrees, then, with that advanced by Mr. Cuthbertson, in pointing out the more oxidable metal as the source of the electricity, which is put in action by Galvanic arrangements. It goes farther, however, and defines that change, which Mr. Cuthbertson was satisfied with terming, in general language, "a loss of metallic property," to be the process of oxidation; and it adds also the important and necessary explanation of the transmission of hydrogen across the fluid of the cells, and the appearance of hydrogen gas at the surface of the copper

plates. In these respects, it is certainly more adequate to account for the phenomena. It is chiefly objectionable, in as much as the data, on which it is founded, are altogether gratuitous. For what other evidence have we, than those very phenomena of the pile, which the theory is brought to explain, that electricity *is* evolved by the oxidation of metals, or that hydrogen *is* capable of forming, with the electric fluid, a combination so little energetic, as to be destroyed by the mere approach of a conducting body? The theory is imperfect, also, in taking no account of that change in the relative quantity of electricity in two metallic plates, which, according to the observations of Bennett and Volta, must necessarily happen when their surfaces are put in apposition.

The discoveries of Mr. Davy, respecting the chemical agencies of the electric fluid, have led him to a theory of the Galvanic pile, intended to reconcile, in some degree, the hypothesis of Volta with that of the philosophers of our own country. It is admitted, by this acute reasoner, that the action of the menstruum, contained in the cells, is absolutely essential to the activity of Galvanic arrangements; and that the two circumstances even bear a proportion to each other. Notwith-

standing this concession, he is disposed to consider the movement of electricity which takes place on the contact of two metals, as the cause originally disturbing the equilibrium; and the chemical changes as secondary, and chiefly as efficient in restoring the balance.

For example, in a pile of copper, zinc and solution of muriate of soda, in its condition of electrical activity, the communicating plates of copper and zinc are in opposite electrical states. And solution of muriate of soda being composed of two series of elements, possessing contrary electrical energies, the negative oxygen and acid are attracted by the zinc, and the positive hydrogen and alkali by the copper. An equilibrium is thus produced, but only for an instant; for muriate of zinc is formed and hydrogen is disengaged. The positive energy of the zinc plates, and the negative energy of the copper ones, are consequently again exerted; and thus the process of electromotion continues, as long as the chemical changes are capable of being carried on.

The most obvious objection, which presents itself against the theory of Mr. Davy, is, that if the chemical agents, forming part of a Galvanic arrangement, be merely effectual in restoring the electric equilibrium, no adequate

source is assigned of that electricity which gives energy to the apparatus. In other words we perceive, in such a process, nothing more than a constant disturbance of the balance of electricity by the action of the plates, and an immediate renewal of it by the agency of the chemical fluids. According to the hypothesis, the production and annihilation of Galvanic energy are carried on in a circle, leaving unexplained that immense evolution of electricity, which is manifested by the most striking effects, both in occasioning the combustion of bodies, and in disuniting the most refractory compounds.

On the whole, the electromotive power of the plates, and the chemical agency of the interposed fluids, appear to be the only circumstances, that can be brought to explain the efficiency of the Galvanic pile. To decide which is to be considered as the cause, and which as the effect, is a difficulty not peculiar to this case, but common to every other, where two events, that are invariably connected, are not distinguished by an appreciable interval of time. The most defensible view of the subject however, seems to me to be that, which attributes the primary excitement of electricity to the chemical changes. But it may be questioned whether the whole of the effect arises

from the oxidizement of the more oxidable metal; and whether it is not essential to the activity of the pile that one at least of the elements of the interposed fluids should be incapable of entering into union with the negative metal. For example, in a pile composed of zinc, copper, and solution of muriate of soda, the oxygen of the water and the muriatic acid, both of which are negative as to their electrical state, are attracted by the zinc, and have their electricities destroyed. But the hydrogen and alkali, having no affinity for copper, except what arises from a difference of electrical habitude, deposit upon that metal a part of their electricity. The electromotive power of the plates now becomes efficient, and determines the current to one end of the apparatus, in the manner already described in a former part of this essay.

Another series of Galvanic phenomena, the explanation of which is attended with some difficulty, are the decompositions that take place in imperfect conductors, forming an interrupted circuit between the two extremities of the arrangement. When two wires, for example, which are inserted into the opposite ends of a tube containing distilled water, are connected with the extremities of the pile, the positive wire, if of an oxidable metal,

becomes oxidized, but if of a non-oxidable metal, oxygen gas is evolved from it, whilst, in both cases, a stream of hydrogen gas proceeds from the negative wire. Why, it may be asked, do the elements of water, thus disunited, arrange themselves at a distance from each other? If the particle of water, which has been decomposed, be imagined to have been in contact with the extremity of the positive wire, the hydrogen must have been transmitted in an invisible state to the negative wire: But if the decomposed water were in contact with the negative pole, then the oxygen must have passed imperceptibly to the positive wire.

These appearances have been explained by Dr. Bostock on the same hypothesis, by which he has accounted for the phenomena of the pile. The electric fluid, he imagines, enters the water by the positive wire, and is there instrumental either in oxidizing the metal or in forming oxygen gas. In either case, the decomposition of the water must furnish hydrogen, which, uniting with the electric fluid, is carried invisibly to the negative pole, the attraction of which for electricity again occasions the separation of hydrogen, and its appearance in a gaseous state. This theory, however, is liable to some objections.

1. It explains the decomposition of those bodies only, which contain hydrogen as one of their elements. And though it has been ably contended by Mr. Sylvester, that the presence of water is, in every case, essential to Galvanic decompositions, yet the fact does not appear to be sufficiently established. Even if it were verified, the agency of moisture might be supposed to consist in its giving that peculiar interrupted transmission, on which the efficacy of Galvanic electricity in disuniting the elements of bodies seems much to depend.

2. If the postulate of Dr. Bostock be granted, that electricity is evolved by oxidation, we shall be entitled to assume the reverse as equally true, viz. that electricity is absorbed when oxygen passes to the state of gas. In cases, where the positive wire is of an oxidable metal, the phenomena accord sufficiently with the theory; for by its oxidation, electricity may be supposed to be liberated, and to form the required combination with hydrogen. But when the positive wire is of a non-oxidable metal, oxygen gas is disengaged; and in the production of this gas the electric fluid might be expected to act, instead of being employed in carrying hydrogen to the negative wire.

The same class of phenomena has been

explained by Mr. Davy on a different theory. According to his view, bodies, which are capable of entering into chemical union, are invariably in opposite electrical states, oxygen for example is negative and hydrogen positive. From the known laws of electrical attraction and repulsion, it will follow that oxygen will be attracted by positive and repelled by negative surfaces, and the contrary process will happen with respect to hydrogen. It is easy, then to conceive that these opposite attractions may produce the decomposition of water. To explain the locomotion of its elements, we may imagine a chain of particles of water, extending from the point P to the point N, fig. 2, and consisting each of an atom of oxygen united to an atom of hydrogen. In fig. 2, the combination is represented as undisturbed, and the chain as consisting of six atoms of water. But when the attractive force of the point P for oxygen, and N for hydrogen, begin to act, an atom of oxygen and another of hydrogen are removed, as shewn by fig. 3, and new combinations happen between the remaining atoms; the second of oxygen uniting with the first of hydrogen, and so on. The terminating atoms being supposed to be removed, a new change will follow similar to the first, and thus the process

will continue to be carried on, not only when the chain of particles is a short one, but when it extends to a very considerable length.

The theory of Mr. Davy, which I have thus attempted to illustrate, derives probability from its being founded on a general property of bodies (their different electrical energies) which appears to be established experimentally, as far at least as experiment can be applied to so delicate a subject. It has the advantage also of explaining a number of facts, chiefly arising out of his own researches, which scarcely admit of being brought under any former generalization. Thus the invisible transference of an element to a considerable distance, even through fluids having a strong affinity for it, (of sulphuric acid for example through liquid ammonia) which is inexplicable on any antecedent theory, is sufficiently explained by this. The ingenious speculation of Dr. Bostock limited the carrying power of electricity to its action on hydrogen, a defect not imputable to him, but to the state of the science at the time when he wrote. Since that period, the discoveries of Mr. Davy have been unfolded by a train of experiment and induction which is probably not surpassed by any thing in the history of the physical sciences, and which will form a durable monument of the genius and industry of their author.

Plate 5.

Fig. 1.

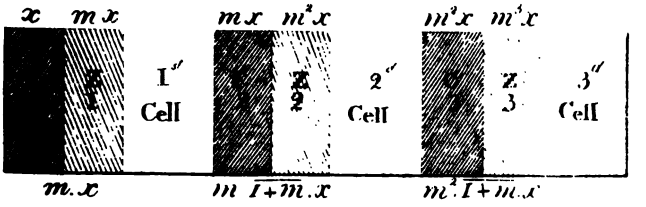


Fig. 2.

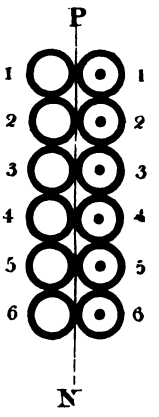
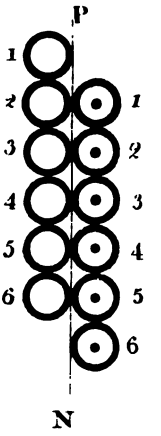


Fig. 3.



Plak

Fig. 1.

Fig. 2.

Fig. 3.

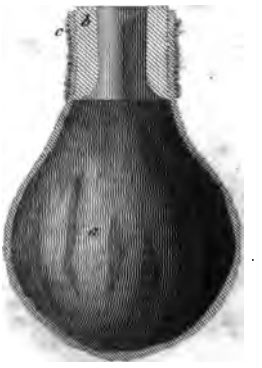


Fig. 4.

Fig. 5.

Fig. 6.

Fig. 7.

Brighton delin.

W Lowry sculp.

CURSORY REMARKS

ON THE

MINERAL SUBSTANCE

Called, in Derbyshire,

ROTTEN-STONE.*

BY

WILLIAM MARTIN, F L. S. &c.

COMMUNICATED BY J. HULL, M. D, F. L. S.

(Read December 28. 1810.)

MR. KIRWAN in his "Elements of Mineralogy," (vol. i. p. 203.) states, that Tripoli is often of pseudo-volcanic and sometimes, perhaps, of genuine volcanic origin;—he adds, however, that "it also frequently arises from the decomposition or disintegration of other stones." The latter observation appears to apply strictly to our Derbyshire *Rotten-stone*, which is usually considered by mineralogists as a *variety* of Tripoli, originating from some unknown decomposed stone of the argillaceous kind. That the substance producing *Rotten-stone* is, however, in its

* *Carieous Anglorum.* Gmel. Linn. Syst. Nat. p. 146.—*Tripoli.* Kirwan. El. Miner. p. 202.

primary state, a *calcareous* and not an *argillaceous* stone, can only be doubted, I think, by those, who have not had an opportunity of examining this fossil in its native repository. Indeed, I feel little hesitation in affirming, that the phenomena, attendant on the substance in question, strongly support the original idea of the late ingenious Mr. Whitehurst, who, from personal and extensive observation, was led to conclude, that the parent rock of the Derbyshire Rotten-stone was *black marble*,* or some other variety of our dark-coloured lime-stones.

It is some years back, since I availed myself of a favourable opportunity, that occurred, of examining the Rotten-stone pits on Bakewell Moor;† and which, I understand, are only opened at particular periods—that is, every third or fourth year, according to the demand, which may then prevail for the fossil as an article of traffic. On looking over the *memoranda*, made at the time of visiting these pits, I find they differ, in some trifling respects,

* Vide Whitehurst's "Inquiry into the original state and formation of the Earth."

† Rotten-stone also occurs at Wardlow Mire; and, as I am informed, at Ashford and some other parts of the county: but I am not acquainted with the local circumstances with which it is attended in those places.

from Mr. Whitehurst's account of the mode, in which Rotten-stone is procured, the appearances it exhibits as a mineral deposit, &c. and, as no late author that I am acquainted with has entered into any detail on these subjects, the following brief statement may not be unacceptable to those, who are interested in geological inquiries.

1. The Rotten-stone, found on Bakewell Moor, is deposited on a limestone, which seemingly belongs to the *first* or *uppermost* stratum.*

2. It occurs in different parts of the moor; frequently on the surface of the limestone, immediately under the vegetable mould; but is procured in the greatest quantity in a long, or somewhat trough-shaped hollow, intersected by several broad irregular fissures, which are filled up with small fragments of limestone—the gravel-like *debris* (rubble) of the traversed stratum.†

3. In these fissures the Rotten-stone occurs at the depth of a few inches below the surface, and from that to ten or fifteen feet.‡

4. It is procured in two distinct states.—In one, the Rotten-stone when dry has an indurated, and sometimes even a stony consist-

* Vide Note A.

† Vide Note B.

‡ Vide Note C.

ence; texture, earthy; fracture, sometimes imperfectly conchoidal; at other times slaty; hardness, from that of chalk to that, which does but just yield to the scraping of the knife (3—6. Kirwan.); feels smooth, sometimes *rather* greasy—never so meagre as the foreign tripoli; does not crumble soon in water; effervesces *slightly* with acids; sp. gr. 2,3. Its colour is usually between a brownish grey and isabella-yellow.—The other variety occurs in a loose or pulverulent form; feels meagre; rarely effervesces with acids; sp. gr. 2,2; its colour light yellowish-grey.

5. The *hard* Rotten-stone (as the indurated kind is called by the Rotten-stone *getters*) occurs in detached, nodular lumps, dispersed through the *rubble* above noticed;—the soft,* as a spongy earth or mud, either *coating* the more indurated variety, or deposited, in considerable quantities, under the *debris*, on the surface of the limestone rock.

6. Water, from the upper part of the moor, is constantly draining through the loose materials, which fill the hollows and fissures of the rotten-stone tract.

7 In this mineral depot are found, with the Rotten-stone, fragments of chert; fragments of a calcareous stone in every possible state,

* Vide Note D.

intermediate between Rotten-stone and perfect limestone; *Rotten-stone with nuclei of solid black limestone;* &c. &c.

8. The calcareous stone, which forms, in these instances, the central parts of the nodular lumps of Rotten-stone, has the *external* characters of the black limestone or marble, found at Ashford-in-the-waters, &c. but differs, somewhat, in its *internal* properties, from any stratum of limestone yet discovered in Derbyshire.

9. Marine reliquia are sometimes found in the hard Rotten-stone; and these are generally such as have been observed to be most frequent in the black marble; viz. *Entomolithus Derbiensis, Conchyliolithus Breynii,* &c. (v. Pet. Derb. T. 45, 39, &c.)

Such are the principal phenomena, which were noted during my examination of the depot of Rotten-stone near Bakewell.—The conclusions, to which this examination led, have been already alluded to; namely, *that Rotten-stone is produced by the disintegration of a particular variety of limestone, probably a black marble;* and that, consequently, authors are incorrect in considering the original substance of this fossil to have been an argillaceous stone.

It will here, however, be asked—how is

the production of this particular substance from another, chemically as well as externally distinct, to be accounted for? and, if Rottenstone be actually the result of a certain change in black marble or limestone, why is it not found in every situation, where such rock occurs? To answer these questions satisfactorily will perhaps be impossible;—to answer them, however, in any way, without having recourse to the reciprocal transmutation of what have hitherto been considered, as simple, elementary parts in mineral compositions,* we must first recur, it is evident, to the nature of the constituent matter of the original rock, as well as of the substance, which the disintegration of such rock has been presumed to produce.

Limestones, it is well known, are composed principally of an indurated calcareous carbonate;—Rottenstone, according to the following analyses, of alumine in a loose or earthy form, and with its constituent particles in a very minute state of division—But we must remem-

* The transmutation of silex into lime, or that of lime into silex or alumine, however strongly contended for by some modern Geologists, most assuredly ought not to be assumed in any attempt to account for the phenomena of the mineral kingdom, till supported by stronger facts than those on which it rests at present.

ber, that many other principles enter into the composition of most limestones besides carbonate of lime; as alumine, silex, bitumen, and sometimes magnesia;—and that Rotten-stone contains, besides alumine, silex, bitumen or carbon, and frequently iron and calcareous earth;—and that the comparative proportions of these component parts differ greatly in the different varieties both of Limestone and Rottenstone.

Our analysis of Rotten-stone has afforded the following results.

1 *Very hard Rotten-stone, approaching Black Limestone in external appearance.*

Alumine	74
Silex	3
Carbonate of Lime	14
Oxide of Iron	2
Inflammable matter and loss	7
	100

2. *Another specimen of the hard variety, but of a light brown colour.*

Alumine	80
Silex	2
Carbonate of Lime	10
Oxide of Iron	1
Inflammable matter and loss	7
	100

3. *Hard Rotten-stone, but less indurated than specimen 2, colour nearly similar.*

Alumine	84
Silex	3
Carbonate of Lime	5
Oxide of Iron	0
Inflammable matter and loss	8
	100

4. *Soft Rotten-stone,* i. e. *with a texture much more loose or earthy than in the other specimens.*

Alumine	87
Silex	4
Carbonate of Lime	0
Oxide of Iron	0
Inflammable matter and loss	9*
	100

* It should be observed that the "*loss,*" in these ana-

If we compare the foregoing analysis with those, which mineralogists have given us of limestones, we shall find, that the chief difference (in a chemical point of view) between Rotten-stone and certain varieties of limestone, exists in the larger proportion of alumine, which the former of these substances contains, and its comparative, or, in some instances, its total want of the carbonate of lime. The particular varieties of limestone now alluded to are those, which Mr. Kirwan has denominated *argilliferous marlites*, on account of their holding a large proportion of *argill (alumine)* in their composition. (v. E. Min. v. 1. p. 99.)—Some of these stones, though

lysis, never exceeded 1,5;—hence the proportion of "*inflammable matter*" may be stated as varying from 5,5. to 7,5. At the time of making my experiments on Rotten-stone, the principal object in view was to ascertain the predominating earth in its composition, and not determining the nature of the inflammable matter, it was placed with the loss;—there can be little doubt, however, of its being *carbon*. Silex was found in all the specimens examined. Carbonate of Lime only in the harder varieties, and not constantly in those. Two or three specimens analysed, in all external respects similar to No. 3, were without it. Oxide of Iron was only present in the harder Rotten-stones.—The actual constituents, therefore, of genuine or perfect Rotten-stone (that is, Rotten-stone in which the disintegration of the original substance is complete) may be stated to be *alumine*, *silex* and *inflammable matter (carbon?)*

affording lime, contain 30 per ct. of alumine, together with small quantities of silex, iron, &c :—and our Derbyshire black marble, or limestone, undoubtedly belongs to this class.—The greatest quantity of this stone is quarried at Ashford-in-the-waters; and, as the quarry is situated at no great distance from the *depot* of Rotten-stone, and affords an excellent example of this formation, I shall here describe the state, in which it is found, and some of its principal varieties. It occurs in beds, which vary in thickness, from a few inches to two or three feet, with interposed seams *(semistrata)* of black, bituminous shale and clay. The substance of these beds, though throughout of the same general aspect, and constantly *burning to lime*, more or less pure, differs greatly in the proportion of its constituent parts, as well as somewhat in its external characters. The limestone of those beds, immediately worked as marble, is of a deep greyish-black, which, on the stones being polished, becomes perfect, or dark-black * :—

* Its colour must be ascribed to the bitumen or carbon, which it contains, as it becomes perfectly white, when calcined, and also acquires a white, or ash-coloured, crust, on exposure to the weather. In many instances I have found the crust of a considerable thickness and become perfect Rotten-stone. And there is no doubt but in walls,

texture close, fine-earthy: fracture slaty, passing into the imperfectly conchoidal *: hardness from 6 to 7 (Kirwan. p. 38.): emits a fetid or rather urinous smell when scraped, but in a much less degree than the following varieties: contains, according to the specimen examined, about 18 per ct. of alumine, with small proportions † of silex, iron, and inflammable matter.

The next variety of limestone, it will be proper to notice, is one rejected by the work-

which are sometimes built of black marble, and in other exposed situations, this would frequently be the case, if a further decay of the stones were not prevented by a timely and friendly covering of lichens and mosses. I have observed, however, that pieces of polished marble, though equally exposed with those in the unpolished state, do not so soon acquire a white crust—Polishing, by filling up the minute interstices, induces a greater degree of external hardness of the stone and prevents for a longer time the decomposition of the surface.

* By *fracture*, is here meant the *general appearance*, or form, which the broken surface of the fossil presents: by *texture*, the grain, or form and disposition of the particles, observable throughout the surface of the *fracture*.

† In no instance did the proportion of silex exceed 4 per ct. or that of the iron $1\frac{1}{2}$. As the experiments, however, which gave these results, were not repeated on each variety of stone, we do not give these proportions as those, which analysis hereafter may find to be correct.—The proportion of alumine, in each instance, we believe, will be found to be near the truth.

men at Ashford, as being less fit for their purpose than that I have just described—It appears to be too soft to receive a lasting polish, and its colour, though black, is much less deep than in the foregoing variety, frequently verging on brownish-black:—texture earthy: fracture slaty: hardness 6: gives out a very fetid smell on being scraped. One specimen of this stone contained, according to the experiments made on it, 66 carbonate of lime; 24 alumine; 1,2 oxide of iron; 1,5 silex; and 7 inflammable matter. Another specimen of this stone, however, *from the same bed*, yielded only 19 alumine.

A third strongly marked variety of limestone, found with the foregoing, has the following characters: colour black, or brownish-black: texture splintery, with disseminated, shining, spar-like particles; these frequently exhibit the minute parts of organic remains: fracture slaty: hardness 7: emits a very fetid odour, when scraped or rubbed. The specimen analyzed gave 8 per ct. alumine, and 4 silex, with 7 inflammable matter, but little or no trace of iron.

It must now be observed, that, along with these three described varieties of limestone, several others occur, which, in their external characters, exhibit various gradations between

the black-marble and the bituminous shale, that separates the calcareous beds; and that the whole formation of these limestone *stratula* appears to graduate, or to pass by an almost insensible transition, into the great *stratum* of shale, under which the limestone of Derbyshire, for the most part, dips.

It is evident, from the above remarks on the black-limestone formation, that among its numerous beds the original of *Rotten-stone* probably exists; and, though the result of my own experiments and observations certainly does not warrant the conclusion, that it has yet been detected as a native rock or stratum, there seems little doubt, but that a more careful examination, than what my leisure when at Ashford permitted *me* to make, may hereafter determine the stone in this state. The variety of black limestone already described, as holding, sometimes, 24 per ct. of alumine, undoubtedly comes near in external characters to the central nodules of marble, which, it has been observed, occur frequently as *nuclei* to the fragments of *hard* Rotten-stone, (v. p. 317.) and which, there is every reason to conclude, are remaining portions of the original calcareous rock. Still, however, this rock appears to have differed essentially from the limestone, with which we are now com-

paring it:—1st. in being a somewhat softer stone. 2d. in containing a much larger proportion of inflammable matter—and, lastly, in holding, at least, 30 per ct. of alumine.* It may here, perhaps, be objected, that a stone, holding even 30 per ct. of alumine, can never be presumed to give by its decomposition a substance, containing more than double such proportion of the material—especially as this substance is evidently *not* composed (in certain instances at least) of the *travelled*, and at length deposited, particles of the original stone; but actually exhibits the matter (in part) of the original stone itself under its primitive structure, and merely deprived of one of the constituent principles.—For this really seems to be the state, in which the greater part of the indurated Rotten-stone occurs. To this objection, I can only, at present, oppose, as probable, the supposition, that, during the formation of *hard* Rotten-stone, while losing the calcareous particles, a gradual and considerable contraction took place in the remaining matter; and that this was effected without destroying the slaty structure, where it previously existed, in the primary stone.† By

* All the specimens I have examined have given something more than the proportion of alumine here stated.

† A nearer approximation of the aluminous particles to

this assumed contraction in the substance of Rotten-stone, it is evident, we may readily account for the greater proportion of alumine it exhibits, on comparing a given quantity of it, with an equal one of limestone.—But it will probably be advanced, that the hypothesis eventually supports more than we wish to prove; as, admitting the contraction of the matter forming Rotten-stone, any limestone holding a *small* quantity of alumine may be

each other may easily be supposed, as a natural consequence of the removal of the calcareous matter; but, that the structure of the original stone should remain, after this loss of matter, will not, perhaps, be as easily supposed or admitted.—However, as the ingredients of black limestones, &c. exist (it is probable) merely in the state of *mixture*, the extraction of any one of these constituent parts will certainly be less liable to destroy the general structure of the stone, than if the process had to act on principles *chemically* united.

We have here considered the structure, or fracture of hard Rotten-stone to be immediately derived, generally speaking, from that of the original limestone; but in some instances, particularly where the slaty structure is present, it is rather, perhaps, the consequence of the *contraction* contended for, than the remains of any particular disposition of particles, which existed in the primary fossil.—We have, not unfrequently, observed the slaty structure in hard Rotten-stone, where no vestige of it appeared in the enclosed *nuclei* of limestone; though it must be observed, that these *nuclei*, in every other respect, were perfectly similar to those, in which such structure was very evident.

the original stone.—The local circumstances, however, attendant on Rotten-stone must prevent such a supposition from being adopted.—All limestones, it is true, are liable to decomposition; and the black *seem* to be more subject to this process * than the lighter.

N. B. *It is to be regretted that this paper was left in an unfinished state, owing to the death of the ingenious author, and that several of the notes, referred to, have not been discovered amongst his manuscripts, though these have been examined with very great care and attention.*

* Vide Note.

objects in directing the mode of dress and other attentions to the person.

In general, the customs formed during the age of barbarism are continued through successive generations, modified by circumstances; but to the custom itself the people are inseparably attached. It is wrong to call it character; it is habit, to which the people are attached. When Ferdinand attempted to assimilate the dress and customs of the Spaniards to those of the French, the people revolted from his government. What character can be given to the transaction, but that of a fondness for national customs, common to every people? and also, when Peter of Russia ordered his subjects to be shaved; although his people loved him as their father, they were unwilling to submit to this supposed degradation, this yielding up an ancient custom. What happened in Scotland when the Highlanders were required to change their dress, is familiarly known to most of us. With such evidence before us, and much more might be adduced, we may infer that national customs are well calculated to keep up national distinctions, and even national animosities; but they do not express the character. The same dignity of office commands equal homage, whatever the costume of that office may be. A Mo-

hawk chief is not less honored than an English magistrate.

Were there an universal standard of taste, the customs of a people might be scrutinized by its laws; but even taste does not govern character; this last rises above and is independent of those things over which taste has any influence. Objects of taste, when applied to character, are what the cornice is to a building; they beautify; but if the people of every country hastily and on insufficient grounds estimate the character of others, the subject has not been overlooked, or neglected, but has exercised the talents of men of vast capacities. Voltaire gives the subject the title of the Philosophy of History; Lord Kaimes, Montesquieu, and Adam Smith, have aided the enquiry; and every historian and political economist, have made national character a leading object of their researches: among whom, Hume holds a conspicuous place.

Every one conversant with the writings of these philosophers, will recollect that they derive national character from religious opinions, civil government, and the state of industry. The subject may be branched into many particulars; but they all resolve themselves into these three points;

in my apprehension, these assigned causes are only consequences. Let us examine the subject. Religion, they say, forms a prominent feature in the character of every people: granted. But religion, having the same object of worship, assimilates its followers; it by no means diversifies their character, however remote their residence; its tendency is to make of one family all nations of the earth; it creates no new principle, nor calls into exercise any new passion; the spirit of devotion is the spirit of filial affection; that act of the mind towards the supreme Being is worship, which exercised towards a parent, is honour and reverence.

But it may be said, that religious principles are acted on only as they are understood, and that persons of different capacities can only understand in proportion to their capacities. This is placing the difference of character not in religion but in the capacities of individuals, which is shifting the ground; but admitting the objection, what does it prove? It proves that the resemblance is incomplete; not that the bent of character produced by religious principles is diversified.

If the pure worship of God be the same in its principle and tendency wherever the worshipper may live, so is its counterpart, superstition. The negroes of Africa, the philosophers of

Athens, the abstemious Bramin, the licentious Turk, vary in the forms of worship; but the spirit of their religion is the same. They all seek to purchase heaven through the agency of a priest. Should a negro become a mahometan, he might change his dress, and perhaps his dinner hour, but the man would be the same; he would not be under the influence of any new motive; he would change his agent, not his character. The question is narrowed to a point; is superstition in its nature the same every where? If so, it must infuse the same spirit and produce the same character. An army is divided into regiments, as the world is into kingdoms; each regiment is known by its dress, its hours of exercise, its peculiar habits and customs; but the character of the regiment is not formed by these fortuitous circumstances. The whole army is led by one general and inspired by one spirit, and the spirit of an army is its character. A nation may worship an ox, or a hero, the sun or a saint, without the slightest shade of difference of character being produced. Let us suppose the same people worshipping these deities in succession; could we in that case discover by the character of the people, which of the deities they were worshipping?

But if national character be not the effect of religious sentiments, is it not decided by the

form of civil government? At a period but little removed from the present, the spirit of the laws, and even the form of worship of all the great states of the continent of Europe were the same. But these strong conspiring causes did not produce an uniformity of character. The French were gay, the Germans grave, the Spaniards dignified, the Portuguese mean, and the Italians base; we must therefore look for some other cause of this contrariety of character.

Small states, by being less secure, are supposed to be mean, cringing and national; and large states, feeling their security, to be oppressive and violent. Should this remark be admitted as correct, it by no means relieves the subject of its difficulties; because there is not a similarity of character in states of the same size, although under the same laws, and observing the same form of religious worship. But before we pursue the subject further, let us consider the extent of the influence of industry on character.

In nations, as well as in individuals, industry appears to be the effect of a previously acquired character, not the origin of it. Rude and uncivilised people are never industrious; industry is the effect, not the cause, of civilization. Industry supposes energy, frugality, and security; it supposes a

fixed government, and a firm individual character. Industry is the wealth of a state and its security. It gives a perpetuity and an impulse to all our blessings. When once in motion it rolls forward, and, like the ocean, surmounts and overwhelms every obstacle. But it is not self-moving; it receives its impulse from wants that are felt, and is an evidence of the state of civilization; but it does not create that state. When we see a luxuriant tree, we attribute its luxuriance to a rich soil and a skilful gardener. In like manner, industry may be attributed to intelligence in the people, and wisdom in the government.

Besides the causes that have been mentioned, climate is commonly considered as having a powerful influence on the character of a people; but a mere glance at history will refute the idea. Men of every character reside in every climate; in the east, the Malays are as brave, and the Chinese as ingenious, as the people of any country. The inhabitants of St. Vincent were courageous to a proverb; and the people of Mexico astonished their discoverers by their attainments in useful knowledge. Climate affects a stranger, but to a native every climate is agreeable, and admits of the developement of his mental energy and corporeal strength. There is no imperfection in the creation of God, but there would be if man

was only adapted to one climate; if another situation changed his character and lessened his consequence.

Should a different plan be adopted, and in place of examining each assigned cause of national character the whole were taken collectively, still we should be as much embarrassed as in ascribing to family character its precise origin; for, nations contiguous to each other, the genius of whose laws and whose religion are the same, are not similar in the leading features of their character: witness, the French and the Spaniards, the Malays, and other nations of India, the original inhabitants of St. Domingo, and of St. Vincent. As we therefore are not able to form a correct estimate of the character of a people by a knowledge of their laws, their religion, or their climate, let us appeal to history.

History is the record of the actions of men; the motives which led to these actions, and the mode of their performance constitute their character; if we were to select a nation, say our own, as an example, and after carefully scrutinising the conduct of the preceding generation, were to state the character of that generation; it is highly probable that the opinion so formed would be correct. If

we were in like manner to unfold the transactions of each succeeding generation, and assign to them their respective characters, it would be evident on comparing them together, that all along the character of the nation was the same, only new circumstances occasioned new expressions of it. If we even turn back to the period of which Tacitus and Cæsar were the historians, and compare the Germans and French of those days, with the Germans and French of the present, we shall discern the same people; and if we take a wider scope, and place before us the maxims of all the rude and barbarous nations that we are made acquainted with, we shall be able to divide them into classes, and to form an estimate of their present and future character. For instance, it is a maxim of most barbarous nations, that theft, and what is always connected with it, lying, are honourable. With other nations, truth and honesty are sanctioned. In the first class we may place the Spartans, the Romans, the Scythians, with all their descendants; and thus we embrace nearly the whole of Europe. In the other class we may place many nations of Africa, perhaps some tribes of America, the Chinese, and the Laplanders. Parke bears ample testimony to the kindness, the integrity, and

truth of some African nations. A mother bewailing the loss of her son, found consolation in reflecting that he never told a lie; no, never. Do we not receive Negroes into our families in full confidence of their honesty? We could not receive a Tartar in the same manner. Dr. Franklin relates that some Indians, noticing the fraud and deception practised by the white people, asked if they had had no mothers to instruct them; evidently implying the office of those of their nation. When referring to the page of history we learn that nations of the first class, when their wants increase beyond their power to supply them, by the robbery of strangers enlarge their views, and that which was called theft is now called war; and he who was the leader of a gang of banditti, is now called general. There is no instance of a nation who in their days of barbarism were great thieves, that did not afterwards make good soldiers. On the other hand those nations whose maxims inculcate honesty, are, at every period of their history, seekers of peace. They do not want courage when forced to exercise it; but they endeavour to avoid the occasion of its being called forth. Hence the Chinese built their wall. Other instances might be advanced to shew how far the

maxims adopted by a people influenced their character; but the present is sufficient for our purpose. One general remark it may however not be improper to make: the maxims adopted by a people carry us beyond the period of their authentic history, and are therefore entitled to much consideration, because they have not their origin, and cannot be enforced by religion or civil government; but they are opinions and voluntarily received by the people, and are an expression of their disposition and character. Hence it appears much safer to argue from the maxims than from any enactment of the legislature, or from any custom that may be followed, and yet they have been almost wholly neglected by enquirers into national character. But with all the aid that can be obtained from history, assisted by the early maxims of a people, the subject is still involved in difficulty; for, there is a striking contrast of character in nations under similar circumstances at the remotest period of their history. To remove this difficulty we must go back to the period when a nation consisted of a small number, and was but as one family; and such a period many nations have known. Thus circumstanced, the father, the patriarch of the family, would inculcate his principles and infuse

his spirit; and hence it is probable the diversified characters of nations have arisen.

Here a most important practical question arises; it has been stated that a nation so pertinaciously adheres to its early received maxims, and so uniformly pursues its first principles in conduct, that the same character ever presents itself. Hence some infer that a child of rude and uncivilized parents, taken from them at its birth, and brought up in the family of an intelligent, well bred European, would both in manners and in mental refinement appear as one of the family. The question to solve is this; would that consequence follow? I presume not. Education, I willingly allow, refines, exalts and assimilates mankind; but no number of the most approved and excellent schoolmasters, would be able to elevate a nation of savages to the rank even of Swedes or Germans in one generation. I do not know that history affords us a precise example of this fact; but there are several which approach towards it, besides many decisive individual cases. Every colony of civilized persons settling among barbarians may be considered as a colony of schoolmasters; but in what instance has a rapid civilization followed? The ancient Germans lived almost under the walls of Rome, and

must have felt their own inferiority. Knowledge, which had elevated the Romans, was in its practical effects exhibited to the Germans; but they were scarcely if at all improved by it. America has been peopled by Europeans more than two centuries; but the aborigines have not received the instruction that was offered to them, and that still continues to be held out. Besides these general facts, many attempts have been made to educate individuals born of uncivilized parents; but no good effect has been produced. The Dutch carried this plan to a considerable extent in attempting to train up young Hottentots in European manners; but the first opportunity that has presented, they gladly threw off their dress, and all the benefits civilization held forth to them, for the filth, the danger, and the wretchedness of their former state. The Americans have trained up young Indians in their principal cities; but they have gone back again to their tribes, filled with contempt at the manners of Europeans. The African society also with the most laudable intention educated many negro children in England; and if I am not misinformed, they ran to a certain level in the acquisition of knowledge, and there became stationary. There was a point, far

below that which European children readily gain, beyond which they could not go. But it is unnecessary to multiply instances; for, there is not a barbarous nation with which Europeans are acquainted, one or more of whose youths have not been trained up and educated with much care in European sentiments and manners; but in every instance without producing a change of disposition. The wilderness and the desert, the tomahawk and the scalping knife, have presented allurements which they could not resist. All they possessed they gladly abandoned; all they had been taught to anticipate, they without hesitation relinquished, and pressed from the crowded city, where all they received was forced upon them, to mix with those who knew no law but their inclination, and whose inclinations were regulated by no principle, but was the mere expression of the passions. Was there only a solitary instance upon record of a child of savage parents, fostered with the utmost care and kindness in a civilized family, being impatient of restraint and hearing of the manners of its parents, endeavoured to imitate them; the subject would not be entitled to consideration. But when every one so circumstanced has resisted civilization, the disposition cannot depend on capricious-

ness, but must have its origin in the nature and constitution of man.

When a pheasant, a wild duck, a hare, or any other undomesticated animal, is attempted to be brought into that state, the effort fails; no person has so tamed a pheasant that it will not, when liberty is given, fly away and not return again; yet the domestication of that species of animals is very practicable. But in order to illustrate the various stages of this process, it may be advisable to select an animal with which we are more familiar. The duck is of this description. It will be granted that wild and tame ducks are of the same species, and differ in no other respect, than that one is domesticated and the other not. In what therefore does domestication consist? It is not in being familiarised to the presence of man; for many have been familiarised without being domesticated. It is a disposition, not a habit; an act of the affections, not the restraint of discipline. A tyger domesticated would be as harmless as a cat; and a cat undomesticated would be as fierce as a tyger. There is no natural propensity in any animal to domesticate. The whole is an effect produced by circumstances. It follows therefore, that there must be a physical change produced on the animal; far from

being alarmed at the presence of man, and untractable, it is attached to his person, and submits to his discipline. As a change of disposition, of constitutional feeling is produced, how is it effected? Let us illustrate the subject by an instance: suppose a pair of wild ducks to be the subject of domestication; they are confined to a yard or a pond, and habituated to the presence of their owner, by whom they are fed and caressed. After a length of time they lose part of their wildness; in this state a nest is formed, and a due number of eggs are laid for a brood of young; but the mother duck is not permitted to sit upon them; they are taken from her and put under a most domestic hen; when the eggs are hatched the hen is unceasing in her attention, informing the young by tones, well understood by them, that they are in safety. But notwithstanding this, the wildness of their nature predominates, and they shun the presence of man, and if not prevented, as soon as they could fly, would take wing and leave the place where they had been brought up. But we will suppose they do not obtain an opportunity to escape, but remain confined to the poultry yard; they are evidently wild, but yet they are not so much so as the old ones that produced the eggs, from which they were

hatched. The discipline and counsel, if I may be allowed the term, of the hen have in a measure softened and corrected their disposition; and being regularly visited without being injured, has also had its effect in lessening their terror at the sight of man. As the summer approaches these also bring forth eggs, which in like manner with the former, are placed under some very tame and familiar hen, and are hatched in due season. The young, like their progenitors, are wild and untractable; but the hen exercises her influence and authority; she persuades and threatens, and some further impression is made; they are not quite so fearful of man as the last brood, but still are eager to escape, and among wild ducks would be as though they had been hatched among them. By pursuing the same plan a few generations more, the object aimed at is obtained; wildness no longer exists; for, a radical change has been effected, not only in the habits, but in the disposition of the animal. The young as soon as hatched are now tame; they require no discipline, no restraint; the building in which they were brought up is their home, and to it they return as the night approaches. So great is the change produced by domestication, that it has the semblance of adding to the world a new

race of animals; the dog that by nature is fierce, like the wolf, becomes the companion and guardian of man; the propensities of the animal have acquired a new bias.

Now what takes place in an animal on its being domesticated, is I apprehend a full illustration of the constitutional, or in other terms, the physical change which passes upon a nation in its progress from the barbarous to the civilized state of society. Perhaps no subject which comes before the political economist is so important as this; and there is no one which he has so entirely overlooked and neglected. It would be very satisfactory to me to enter fully into the subject, and by an appeal to history, to establish the sentiment advanced; but the rules of the society prevent my taking more than a glance of the subject at present.

A nation in a state of barbarism, remains age after age, without any variation in their manners, or any improvement whatever, unless some circumstance arises to compel a change. The circumstance which in every instance has been instrumental to this purpose is, an increase of population. The rivers and the forest have not afforded a sufficiency of food, in consequence of which agriculture, in a rude manner, is commenced; and tribes

which had been wandering now become stationary. The seed their hands had planted requires their presence to protect it.* Thus an important point is gained, and a new æra commences; the wives and children are in greater safety; consequently the families become larger and require an increase of industry to provide the means of subsistence. The effort this requires enlarges the ideas and encreases the knowledge of the people; and, after a succession of generations, their habits and their constitutional propensities change; they no longer delight in the practices their ancestors were attached to; having passed

* Mr. Malthus in his work on population, asserts that when the population of a state has encreased beyond the existing means of subsistence, the superabundant part must be removed; he appears not to have taken into his consideration the possibility of a change of system, and the effect that change may have on the produce of the soil, and on the fecundity of the people; but especially he does not apprehend that an increased population is the great agent for the civilization of mankind; no people have ever increased in civilization in consequence of wealth, abundance, and a thin population, but as the effect of an increase of industry, and industry is the creature of want, supposed or real. There is not enough, and therefore individuals labour to obtain more; and by this effort their mental energies are roused, and they go forward. I have no hesitation in stating that there is no progress in civilization, but what is compelled by the very circumstance which Mr. Malthus lays down as the foundation of human misery, an increasing population.

from the savage to the agricultural state of society, they are now passing on to the next step of their progress, the imitative state. Every change here noticed has been effected by the natural consequence of an increase of population, as the history of the world bears ample testimony; indeed every page records the fact, that progress in civilization and in population correspond, and are cause and effect. Ascertain the one, and a correct judgment may be formed of the other.

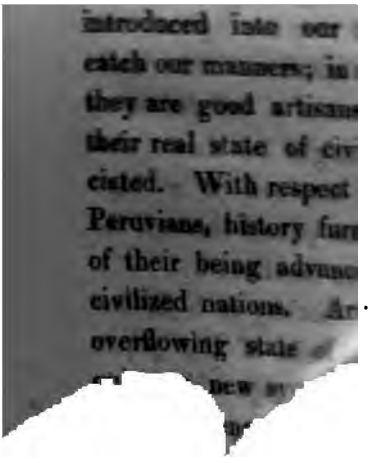
Let me here call the disciples of Mr. Malthus to a consideration of this subject, and to a candid enquiry whether what that gentleman has held forth to the world as its great curse, is not its greatest political blessing. That there is fixed in the nature and constitution of man a check by which the unlimited increase of the species is prevented is readily acknowledged; civilization is that check. If we banish war, famine and pestilence (and it is in the power of man so to do,) and let population roll forward with its utmost speed, the effect will be to dignify man by the expansion of his faculties. But as this takes place he becomes less of the animal, and the average number of children to a marriage sink: if they are five at a given period, a little increasc of population and its

consequent civilization, sinks the number to four. Such is the testimony of the registers of nations, and not that disheartening sentiment Mr. Malthus makes them speak.

. We have conducted the human race from the agricultural to the imitative stage of civilization; let us view him in that situation. In the purely agricultural state the faculties of man are dull, and it is difficult to excite an interest in any new pursuit. They are agriculturalists merely to procure the means of subsistence, having no relish for mental pursuits. But when they have burst this barrier, they see with delight what nations more civilized have effected, and they strive to imitate them; and it is at this stage of civilization, that the imitative powers of man are by far the strongest. It is now that nations undertake those stupendous works which astonish future generations. There is little envy among them; for, there is no invention. They are pleased because they can imitate, and thus claim a connection with those to whom they look with admiration and respect. If at the lowest link of the chain we place the New Hollander, and designate him by the name of savage, if at the next advance we find the Otaheitean, and many tribes of Americans, people whose business it

is to procure the means of subsistence, and to injure their neighbours; we come next to the point at which the human faculties begin to unfold, and the man to appear; when the malignant passions, which he had nursed in a state of barbarism, now give way, and he begins to seek for rank and consequence among civilized nations. In this stage of civilization are the Russians, the Negroes, the Mexicans and the Peruvians.

Dr. Clarke in his account of the Russians, lately published, describes their imitative powers as most astonishingly great. A painting of the most exquisite art, they copy with so much accuracy, that even with a good judge it passes for the original; and this capacity for imitation embraces every object, whether of the most exquisite or of the rudest structure; but they invent nothing. Many Russian youths have been instructed by the best masters in their own nation, and in foreign universities; but there has never yet been a book written by a Russian, worth translating into another language, or the smallest improvement made by them in any art or science. Their judgment is weak; give them a written description, and they would not comprehend it; but place before them a model, and they will without hesitation undertake to



many respects like the father of our country, the great king Alfred, was placed among them; he taught them the arts of civilized life; and the whole nation at once imitated them, so that when the Spanish ships arrived on their coast, drawings of them were made and sent by post to Mexico. But the history of that period is known to you, Gentlemen. In referring to it you have only to ask the question, whether that people were not as far advanced in civilization as the Russians are now, and whether their civilization was not of the same description; whether it did not consist in imitation. When a nation has remained several generations in this degree of refinement, and the population again presses forward, further advances are made. The mind becomes stronger as it is more exercised, till step after step the highest, and the best state of man is attained. The limits of an essay, do not admit of a full discussion of the subject, or it might be shewn that every nation that has attained to a high degree of civilization, has passed through the gradations that have been mentioned.

I must also call the society to a farther consideration than the limits of this paper will admit, of the physical change which civilization produces on its subject; a change

only to be effected by many generations, but which when once accomplished is permanent; so that a nation, when it has attained a degree of civilization, never loses it; it becomes part of the constitution, I may say, of the nature of the man; in the same way as domestication becomes part of the nature or constitution of an animal. A people may become stationary; they may become ignorant; but they never a second time become savages.

OBSERVATIONS

ON THE

EBBING AND FLOWING WELL,

At Giggleswick, in the West Riding of Yorkshire,

WITH A THEORY OF

RECIPROCATING FOUNTAINS;

BY Mr. JOHN GOUGH.

IN A LETTER TO DR. HOLME.

(Read October 4th. 1811.)

SIR, Middleshaw, near Kendal, July 22, 1811.

I ADDRESSED a letter to you on reciprocating fountains, in February 1806; which you did me the honour to lay before the Literary and Philosophical Society of Manchester, on the seventeenth of October following. Certain additional facts relating to the subject have come to my knowledge since that time; the importance of which has induced me to supersede my former communication by a corrected essay on these singular phenomena.

When a theory happens to be formed from the comparison of a few facts only, future observations frequently perplex it with diffi-

culties, which are not easily surmounted. It is not necessary to seek for examples to corroborate the preceding assertion; for, in all probability, most philosophers will be able to establish the truth of it, by incidents which are preserved in the private histories of their own speculations. In my opinion, however, the writers on Hydraulics furnish a striking instance of the fact in the machinery, which they commonly employ for the purpose of explaining the causes of reciprocating fountains, or of ebbing and flowing wells as they are called in vulgar language.

Springs of this description may be reckoned amongst the rare productions of nature; the infrequency of which leads me to conclude, that but few thinking men have had an opportunity of observing a number of them with attention, and of comparing their operations; for it is certain, that by far the greatest part of the world knows nothing of the subject, except by report. This want of ocular information, in all probability, has obliged speculative writers to rest content with the few facts, which are to be found in books; and I am only acquainted with the following narratives, which can be said to throw any light on the curious properties of reciprocating fountains. The first that I shall mention,

came from the pen of the younger Pliny; who flourished as a statesman and a man of letters in the time of Trajan. The account may be found in the concluding letter of the fourth book of his epistles; and the following is an attempt to give it in my own language, as I have no translation of the work in my possession.

PLINY to LICINIUS. "I am going to pre-
"sent you with a description of a natural cu-
"riosity in the neighbourhood of my country
"house, in hopes that it will prove an interest-
"ing speculation to a person of your extraor-
"dinary attainments. A spring rises on the side
"of a mountain, and runs along a rocky chan-
"nel into an artificial basin placed in a summer-
"house, where it is for some time detained, and
"then falls into the Larian Lake. This foun-
"tain possesses a surprising property; for it
"flows and ebbs thrice a day, observing a
"stated law of increase and decrease. This
"singular circumstance, may be observed with
"ease, and is calculated to amuse the specta-
"tor. You may sit in the apartment, make a
"slight repast, and drink of the water of the
"fountain; which is deliciously cool. In the
"mean time the reciprocating motion of the
"spring proceeds equally, and in a manner
"which is easily ascertained, by placing a

"ring, or any other small object, upon a dry "part of the basin. The water will rise "gradually to the mark, and afterwards cover "it. The fountain will, at length subside, so "as to leave the object dry; and will be after- "wards seen to retire slowly. If you pro- "long your stay, these alternate motions will "be repeated two or three times. Is this "singular appearance occasioned by air act- "ing upon the outlet of the fountain; so as "to obstruct the current, when it enters by "the mouth of this channel, and, after its "escape to allow the water to issue more "freely? We know this to be the case "with bottles, and all kind of vessels, which "have narrow necks: for when they are "placed in a position proper for discharging "their contents, the resistance of the air "makes them guggle, and the liquor issues "from them in an interrupted stream. Or, "does this fountain partake of the nature of "the ocean? Is its current retarded at one "time, and accelerated at another by the "causes, which give rise to the flux and "reflux of the sea? Rivers we know are "driven back, when they fall into the sea "against the wind and tide. May not some "cause, in like manner, periodically obstruct "the discharge of this fountain? Or, are we

"to suppose, that the subterranean veins of "the fountain have a certain capacity; and "that while they are recruiting their ex- "hausted stores, the stream is small and "languid; but becomes stronger and more "abundant, when these reservoirs are reple- "nished? Or is there a secret and unknown "contrivance of a stop acting on the prin- "ciple of a balance; which accelerates the "efflux of the fountain while it empties itself, "and diminishes the current, while it is "filling?"

The two last suppositions are obscurely expressed in the original; the latter of them however seems to have suggested the hypothesis of a rocking stone; which acting on the principle of a valve, alternately opens and shuts the out-let of the spring; and my translation is made to favour this conjecture. The elder Pliny also mentions the same fountain, and ascribes to it a very remarkable and unaccountable difference; for he asserts, that it ebbs and flows regularly in the space of an hour. HIST. NAT. *Lib.* II. *Cap.* ciii. We are surprised to find the uncle and nephew, both intelligent and observing men, very so widely in the statement of an obvious fact. Their disagreement however does not contradict the regularity of the spring's operations; which is

a consideration of importance, in the natural history of reciprocating fountains. As for the question of accuracy, it has been decided in the uncle's favour by Catanaeus, the learned commentator on the epistles of the nephew; who says, the fountain continued to reciprocate in his time, that the neighbours called it Pliny's well, and that it answered to the description given of it, by the elder writer of that name. After all, future observations may prove, both these authors to be in the right. Perhaps it will be found, that wet weather accelerates the reciprocations of the spring, by increasing its discharges; while a dry season diminishes the efflux of water, and makes the fountain more dilatory in its operations. The preceding conjecture is countenanced by the reciprocating spring at Giggleswick; for it ebbs and flows most frequently after copious rains; but the depth of the well shews the greatest variations, when the efflux is but small.

The elder Pliny also takes notice of another reciprocating spring, and gives the following short character of it with his usual brevity. "The fountain of Jupiter, in Dodona, ex-"tinguishes lighted tapers like any other cold "water; but if a taper be first extinguished, "and then brought to the surface of the well,

"it takes fire again. This fountain is called "ANAΠΑΥΟΜΕΝΟΣ that is, the *Loiterer;* be- "cause it is empty at noon; but beginning "to increase after mid-day, it overflows in "the middle of the night, and then subsides "again gradually." HIST. NAT. *lib.* II. *cap.* ciii.

A third extraordinary fountain of this kind is mentioned by various modern authors. It is said to be in Paderborn a district of Westphalia, and to go by the name of Bolder-born, or the boisterous brook. This is an appellation which it deserves; for after flowing twenty-four hours, it ceases for six hours; at the end of which period, it returns with a great noise and force sufficient to turn three mills, situated near its visible source. The operations of this fountain are differently described in the Philosophical Transactions, where it is said to lose itself twice in twenty-four hours; coming always after six hours back again. Lowthorp's abridgment, Vol. II. Page 305.

The prevailing opinion, respecting the nature of reciprocating fountains, appears to be derived from the three preceding instances; at least, I am not acquainted with any other topographical account, which can be said to favour the notion on rational, or even on pro-

bable principles. This theory may be found in many popular works on natural philosophy; and it is easily explained by the hydraulic machine called Tantalus's Cup. This instrument consists of a vessel furnished with a siphon, which may be attached to it in different ways. To avoid the necessity of a diagram, we will suppose the bottom of the vessel to be perforated, and the longer leg of the siphon to pass through the hole, being firmly cemented in a position, which places the highest point of the bend within the vessel, and half an inch or an inch below the brim, and at the same time keeps the open or lower end of the shorter leg at a small distance from the cup's bottom. Water flows through a tube in an uniform stream into the cup; where it is collected for want of egress, and entering the siphon at the open end of the shorter leg, it rises gradually to the bend or highest point. The subsequent rise of the water in the cup, forces the column in the ascending leg of the siphon, to pass over into the descending or longer branch; upon which this instrument begins to act, not in the manner of a simple tube, but in its proper character. Now the draft of the siphon is made to exceed the opposite stream or supply of water; in consequence

of which contrivance the cup is emptied again sooner or later; at this moment the action of the siphon is suspended, until the cup is replenished by the constant current. In this manner the water will be seen rising and falling alternately in the cup, which will be full and empty, or nearly so, by turns. Similar vicissitudes will also take place in the siphon; for it will run, so long as its shorter leg is in the water, and then stop, until the highest point of the bend is again covered by the contents of the cup.

The transition is easily made from Tantalus's cup to a fountain, which reciprocates periodically; for we have only to suppose a secret reservoir to be formed in the bowels of a mountain on the principles of this instrument, and the following appearances will take place in the visible well, which receives the water from the natural siphon. 1st. So soon as the surface of the pool in the subterranean reservoir, rises above the bend of the siphon, this canal will begin to act; and its discharge will be greater at that moment than at any other period; because the power of a siphon is greatest, when the distance, betwixt the bend and the surface of the water in the basin, is least. 2d. This abundant influx into the external well will make it rise; in conse-

quence of which the efflux will continue to encrease at the outlet, so long as the water continues to accumulate in the visible basin. 3d. Now the discharge from the outlet, which becomes more copious every moment, being contrary to the influx from the siphon, which grows gradually weaker, the surface of the well will cease to rise so soon as these opposite powers are equal in their effects; and the flow will be at the full in this instant. 4th. The well cannot remain stationary, for any length of time, at its highest elevation; because the vigour of the siphon being perpetually on the decline, all the water discharged by it will run off through the outlet, together with part of that, which had been previously accumulated in the visible fountain, during the time of the flow. 5th. Hence it is evident, that the well will begin to subside, the moment it becomes stationary; after which it will persevere in a retrograde motion, until the siphon shall have emptied the subterranean reservoir. 6th. If no veins of water discharge themselves into the visible basin, besides the siphon which runs periodically, the spring is called, an INTERMITTING fountain. The Bolderborn is of this kind, for it remains dry while the secret reservoir is filling, and flows while the siphon is in action.

7th. But if the spring receives other supplies in addition to the intermitting current, it is called a RECIPROCATING fountain; because the stream that issues from the outlet of the visible basin is permanent, though it varies in quantity; on this account the well ebbs and flows alternately, but never runs itself dry. All the fountains, which will be mentioned in the sequel, are of this kind; and Pliny's well, near Coma, appears to possess the same character from his description of it. 8th. The fluctuations of an ebbing and flowing well, which is fed by a siphon, will remain invariable, so long as the stream, that falls into the subterranean reservoir continues to be uniform. But these external and visible operations of the well, are so far under the influence of the current last mentioned, that they will evidently suffer a temporary suspension, so often as the influx into the concealed cistern amounts to a certain quantity in a certain time; for the siphon is but a secondary agent in producing the phenomena of reciprocation, its business being to empty the subterranean basin, so often as it is replenished. Now the time of filling this magazine of water will be the shortest, when the influx into it is most abundant, and the contrary, consequently an increased discharge into the subterranean re-

servoir, will diminish the intervals of the siphon's inactivity, and prolong the periods of its action. It follows from these premises, that when the influx becomes equal to the feeblest effort of the siphon, the quantity of water thrown into the concealed basin, will exactly counterbalance the quantity which is drawn off by the crooked canal; and the external *well* will assume the character of a common fountain under these circumstances.

I have now explained the principles, on which the common theory of reciprocating springs is founded; and the necessary consequences of the theory are stated in the eight preceding propositions. This has been done, to shew with what ease a natural apparatus on the construction of Tantalus's cup elucidates the appearances, which have been ascribed by writers to the fountains of Dodona, Coma, and Paderborn. The operations of these springs are happily illustrated by the instrument in question; on which account I do not hesitate to pronounce the theory to be a good one, so far as it relates to these fountains alone; provided they are faithfully described. The simplicity of the preceding explanation and its coincidence, with the narratives of the two Pliny's, as well as the history of the inconstant brook in Westphalia, disposed me to

admit the common theory, and to imagine it to be equally applicable to reciprocating fountains in general; until an instance occurred to my notice, which proved that, fluctuating fountains do not universally exhibit the periodical operations which are described by the writers already quoted. I made a visit to Giggleswick Well in the autumn of 1796; which taught me to value this once favourite theory not so highly, and in particular to dispute the universality of its application. The causes of these doubts will be easily perceived from the following description of the well and its operations.

This spring lies at the foot of Giggleswick Scar, which is a hill of limestone in the West Riding of Yorkshire. The water discharged by it, falls immediately into a stone trough; in the front of which are two holes near the bottom; these are the outlets of two streams, that flow constantly from the artificial cistern. An oblong notch is also cut in the same side of the trough; which extends from the brim of it, nearly to the level of the two holes already mentioned. This aperture is intended to shew the fluctuations of the well: for the water subsides in it, when the stream issuing from the rock becomes languid; on the contrary the surface of the

water rises again in the notch, so soon as the influx into the trough begins to be more copious. The reciprocations of the spring are easily observed by this contrivance; and they appear to be very irregular both in respect of duration and magnitude. For the interval of time betwixt any two succeeding flows, is sometimes greater, and at other times less, than a similar interval which the observer may happen to take for his standard of comparison. The rise of the water in the cistern, during the time of the well's flowing, is also equally uncertain; for it varies from one inch, to nine or ten inches, in the course of a few reciprocations. It is necessary to remark on the present occasion, that the spring discharges bubbles of air, more or less copiously into the trough; these appear in the greatest abundance at the commencement of a flow, and cease during the ebb, or at least issue from the rock very sparingly at that time. In fact the appearance and disappearance of these bubbles, are circumstances equally inconstant with the rise and fall of the water.

The irregularities exhibited by the ebbing and flowing well, during my short visit, diminished the respect which I formerly had for the popular theory, more especially when consi-

dered as a general explanation of reciprocating springs. This change of opinion was suggested by the caprices of the well; which were too many and too singular to be ascribed to the uniform operations of a single siphon, as we have seen already; and the accidental combination of several siphons in one fountain, is a conjecture too improbable in itself to demand a serious discussion. My suspicions respecting the accuracy of the principle were not a little increased, by the following descriptions of two reciprocating fountains. Weeding Well in Derbyshire, appears to be more fickle and uncertain in its reciprocations, than the well at Gigglewick. Dr. Plot describes this remarkable fountain, at page 48 of his history of Staffordshire, where he reports it to be very uncertain in its motions, ebbing and flowing sometimes thrice in an hour, and at other times not oftener than once in a month: he also quotes the following character of it, to the same import, from a Latin poem by Mr. Hobbs.

" Fons hic temporibus nec tollitur (ut Mare) certis;
" Æstibus his nullam præfigit Ephemeris horam."

The following account of a reciprocating fountain is extracted from an article in the second volume of Lowthorp's abridgement,

page 305; in which care has been taken to preserve the facts recorded by the author, Dr. W. Oliver, in language more concise than his own. "Lay Well, near Torbay, is about "six feet long, five feet broad, and near six "inches deep; it ebbs and flows very visibly; "and many times in an hour. The recipro- "cations succeed each other more rapidly "when the well is full, than they do when it "is low. When once the fountain began to "flow, it performed its flux and reflux in "little more than a minute's time; but the "Doctor observed it to stand sometimes two "or three minutes at its lowest ebb; so that "it ebbed and flowed about 16 times in an "hour, by his watch. So soon as the water "began to rise in the well, he saw a great "number of bubbles ascend from the bottom; "but when the water began to fall, the bub- "bling ceased immediately. The Doctor "measured the distance betwixt the high and "low water marks, not on a perpendicular "line but on a slope, and found it exceeded "5 inches."

The three preceding instances of irregular reciprocation undoubtedly diminishes the importance of the popular theory, by proving that it is not of universal application; as it only explains the constitution of those foun-

tains, which ebb and flow periodically. The Bolderborn of Westphalia, may be reasonably pronounced to be of this description; as for the fountain of Jupiter in Dodona, we know too little of it to judge of its true character; and it is not improbable but future observations will add Pliny's Well to the class of irregular reciprocators.

It may be reasonably supposed, that since I have endeavoured to confine the established theory of reciprocation to one or two springs at most, a new explanation will be offered on my part, comprehending the phenomena of those wells, which ebb and flow according to no certain rule. Before I make this attempt, it will be proper to give a more circumstantial account of the appearances exhibited by the well at Giggleswick, than has hitherto been published. I neglected, when in the country, to preserve a correct register of its fluctuations, and committed no other observations to writing, except those which appear in a former part of this essay. This omission, however, has been fully supplied by Mr. John Swainston, of Kendal; to whom I formerly communicated my imperfect remarks on this well, requesting him at the same time to note down a series of its operations, at some convenient

opportunity. This request was complied with by my friend; who has digested his observations in the following table, which merits the esteem of the naturalist, as being a faithful history of this singular fountain.

Observations made on Giggleswick Well, August 20th, 1804, *from* 3 *to nearly* 6 P. M.

On first coming to the Well it continued flowing near ten minutes, and then as in the Table.

No. of inches ebbed.	Time in Ebbing in minutes.	Stationary at Ebb in minutes	No. of inches Flowed.	Time in flowing in minutes	Stationary at flow in minutes.	
8½	4	7½	9	2	1½	
1	1	—	½	—	1	
—	—	—	½	—	—	
1½	—	—	½	—	—	
9½	4½	3	9½	4	2	×
1	3	—	½	—	2	
5½	3½	—	7	1	1	
½	—	1	—	—	—	
3	2	—	4	3	4	Bason 1 inch short of full.
6	3	—	7½	1½	1	
6½	3	none	6	1	2½	
6½	3½	—	7½	1½	1½	full
9	4½	2½	9	2	2	
9½	4½	5½	9½	3½	1½	×
½	½	3	—	—	—	
1	—	3	—	—	—	
5	2½	none	6½	1½	Left it flowing over.	

Mr. Swainston has favoured me with the following explanatory remarks; which perhaps will throw some additional light on the history and properties of Giggleswick Well. In the two observations marked with crosses,

the water flowed slowly for the first 3 or 4 inches, and then rose very quickly, until the cistern was full; the same appearance took place not unfrequently in the course of his remarks. Where the blanks are in the columns marked stationary at ebb, the water flowed again instantaneously; but there are some inaccuracies in this part of the table; for Mr. Swainston was interrupted more than once by travellers stopping to let their horses drink. The term stationary at ebb, signifies that the surface of the water in the cistern was stationary at its lowest elevation; at which time the discharge from the trough was commonly confined to the two holes near the bottom of it.

I have now stated all the facts in my possession, that relate to reciprocating springs. The fountains, which have been described, are six in number, of these the inconstant brook in Westphalia, appears to require the agency of a siphon to account for its operations. The characters as ascribed to Pliny's Well, and the well in Dodona, are very ambiguous and unsatisfactory: but the operations of the three remaining springs, and more especially the register of Giggleswick Well, perplex the hypothesis of a siphon with insuperable

difficulties; which a superficial inspection of the table will discover to the reader.

The theory, which I shall now propose for the explanation of irregular reciprocating springs, was suggested by an accidental observation, which occurred to Mr. Swainston, whom I have mentioned above. This Gentleman, who is a manufacturer of Morocco-leather, has a contrivance in his works, for the purpose of filling a boiler of a particular construction with water. This apparatus consists of a tub, which is elevated considerably above the boiler. The water is conveyed from a pump along a trough into this vessel; from which it runs immediately into the upper extremity of an inverted siphon, which is cemented into a hole in the bottom. This compound tube consists of three branches or legs; the first descends perpendicularly beneath the tub, and is the longest of the three; the second ascends again and carries the water, which comes into it from the first, to a convenient height above the brim of the boiler; the third is a descending leg, which performs the office of a nozle, that is, it discharges the water from this crooked canal into the boiler. Mr. Swainston observed by accident, that when the workmen were filling the vessel last mentioned, the water reciprocated in the tub, the

surface of it rising and falling alternately in a manner which he could not explain, by supposing some slight irregularity in the management of the pump. When the appearance was more carefully examined, he found a corresponding variation in the efflux at the nozle; for when the water was rising in the tub, the stream was perceptibly weaker at this outlet, than it was during the ebb or fall of the water in the vessel last mentioned. He farther observed, that when the water in the boiler rose high enough to cover the end of nozle of the siphon, bubbles of air were seen ascending from this orifice, during the ebb in the tub, or at least during the former part of it; but that they did not appear during the flow, or whilst the water was accumulating in the tub. The fluctuations here described, were far from being regular, either in magnitude or duration; for the water rose much higher in the tub at one time, than it did at another; and the intervals betwixt flow and flow, or ebb and ebb, were very unequal. In fact the appearances seen in this vessel imitated the caprices and singularities of Giggleswick Well in a natural and surprising manner.

The exact coincidence of the effects, produced by an artificial apparatus, and a noted

reciprocating fountain, will naturally turn the attention of the curious to inquire into the cause of the irregular motions, which Mr. Swainston observed in his reservoir. The circumstance on which these fluctuations depended, is easily understood; for, seeing the inverted siphon discharged bubbles of air occasionally into the boiler, it is manifest that this subtle fluid entered the tube, mixed with the water, or in other words in the state of foam. Now it is well known, that the bubbles, constituting this frothy substance burst, and the air separates from the water, when the agitation ceases; by which the compound was produced. Such a separation would take place unavoidably in the siphon; because a current flowing in a tube moves on smoothly, or without interruption which is the cause of agitation. The process here described, discovers the nature of the phenomena which are exhibited by Mr. Swainston's vessel; for the air, which separates from the water in the siphon, is collected in some part of that tube, most probably in a bend connecting two adjacent legs; where it forms a bubble or mass, large enough to produce a considerable obstruction in the current, by contracting the area of the pipe. The water will evidently rise in the tub, so long as its efflux is inter-

rupted by this obstruction; but the action of the stream in the siphon will push the mass of air from place to place in its own direction until it shall be discharged at the nosle. The removal of this impediment will restore the stream to its full vigour; upon which the water will begin to subside in the tub; and it will continue to do so, until the surface arrives at its proper level; unless a second collection of air happens to be formed in the mean time. We have now investigated the nature of the reciprocation, observable in Mr. Swainston's apparatus, it proceeds entirely from the obstruction of air bubbles, lodged in the crooked canal; the formation of which depends on causes that act in a fortuitous or irregular manner; consequently the reciprocation which results from their united operations will prove to be equally uncertain and variable.

Should the preceding theory of an ebbing and flowing vessel receive the reader's approbation, he will be disposed to think, that Pliny discovered the true nature of reciprocating fountains, when he compared the fluctuations of these springs to the interrupted and irregular stream, which issues from a bottle. In fact, only one circumstance seems wanting to render his explanation of the phenomenon

complete; he has not informed his friend Licinius, how he supposes the air gets into the subterranean channel, which supplies his well with water. Perhaps this omission was the effect of design, rather than of negligence; for many philosophers in Pliny's time held the singular opinion, that the earth possesses the faculty of respiration like animals; in consequence of which it inhales and expires air through the crannies and caverns, which extend to its surface. Supposing Licinius to be of this way of thinking, Pliny had no reason to tell this ingenious and learned man, that he imagined the outlet of the fountain had a communication under ground, with one of these spiracles of the globe. Be this as it may, the notion is too absurd to be mentioned in the present improved state of Natural Philosophy, in any other light than as a curious document of the puerile conceits with which the philosophers of ancient times amused their hearers. In the foregoing attempt to complete the theory, I have had recourse to a well known phenomenon; water is beaten into foam by being agitated; which was the case with Mr. Swainston's vessel, because a strong current fell into it from the pump. There is, however, one objection still remaining, which deserves to

be considered: the levity of foam, compared with the superior weight of water, may lead some persons to suspect, that this light substance will not mix with water, but will float on the surface of the reservoir, in which it is formed. Supposing this suspicion to be well-founded for the sake of argument, we must allow the foregoing theory of reciprocating vessels to be defective in a very essential point; because if foam cannot sink, the air, that proceeds from it, cannot find its way into the tubes or siphons, which convey the water from such vessels. Being unwilling to leave this objection unanswered, I resolved to put the truth of this principle to the test of direct experiment; which was done in the following simple manner. A small bell glass, being first filled with water, was inverted in six quarts of the same fluid, contained in a small tub. Things being thus prepared, the contents of the open vessel were agitated briskly; and the air which entered the water, found its way into the inverted glass, the upper part of which it occupied. The water of the tub was agitated by the motion of a whisk, or a bundle of slender twigs; it was sometimes taken up in a pitcher, and returned into the vessel quickly, from the height of a foot or more:

both methods proved successful, but the former appeared to introduce air into the glass with more expedition than the latter did; the difference here mentioned, may however depend entirely upon management and accidental circumstances. The experiment which I have now related, shews the foregoing objection to be of no moment; consequently the present theory of irregular reciprocation may be pronounced to stand upon a safe foundation, and unexceptionable principles.

The observations which have been made on Mr. Swainston's accidental discovery, render an elaborate inquiry into the constitution of Giggleswick Well unnecessary. Nature may be easily supposed to have produced an apparatus in the side of the hill, possessing the mechanical properties of the reciprocating tub, and all the phenomena will follow; which are so remarkable in this fountain. Let us imagine a reservoir to be concealed from view under the rocks; into which the stream of a subterranean brook falls, and beats part of its contents into foam by agitation. Let this cavity be connected with the external or visible basin, by a narrow serpentine chink concealed in the interposing strata; and the reader must perceive without farther explanation, that this conduit will

perform the part of the inverted siphon already described, and exhibit the operations as well as the irregularities of the fountain in question. The same internal structure may be supposed to exist in Lay Well, near Torbay; but something is required in addition to this simple apparatus to account for the casual reciprocation of Weeding Well, in Derbyshire. It is not a difficult task to accommodate the theory to the description of this spring; but when we consider how imperfect such descriptions are commonly found to be, it appears more advisable to pass over this fountain in silence; until some accurate observer shall present the public with a correct and minute history of its operations.

All parties allow, that reciprocating fountains flow from pools of water, concealed under ground; on which account it will not be very foreign to the topic of the present essay, if I conclude it with a few remarks on the structure and formation of caverns. I have visited many caves in this part of England; all of which are situated in the strata of calcareous hills. They also appear to have been once filled with an argillaceous stone, of a less durable nature than the surrounding limestone. This supposition is corroborated

by the following fact; masses of clay, mixed with gravel, are found scattered up and down these hollows; and as they are lodged in chinks from which they cannot be easily removed by water, I suppose them to be the remains of extensive beds, which formerly occupied these recesses in the calcareous strata. This argillaceous matter, which choaked up the natural vaults of our limestone hills in early ages, has been gradually worn away by a simple, but powerful agent. The rains which have fallen from the remotest times, constantly find their way through the chinks of the limestone; thus subterranean brooks were formed, which attacked the soft argillaceous matter, situated under the harder covering of limestone. This perishable substance was first softened by the water; and afterwards broken down by the currents; which washed away the clay and gravel. In consequence of this alteration, the incumbent rocks of limestone were left to rely on themselves; such therefore fell down, as were not supported by mutual pressure; while the rest still remain suspended in the roof and sides of the caverns, being locked together like the stones of an arch. The agents, which were formerly employed in the excavation of those subterranean chambers, remain in many in-

stances to the present day; for almost every cavern is the place of union to a number of secret brooks, which enter it in different directions, some of them being perennial, but others depend on the weather. The impetuosity of these currents is very apparent in some caverns, which are filled with water in wet seasons; for the bottoms of them are covered with large masses of stone; the edges and angles of which are worn away, like those of a pebble, that has been rolled in the channel of a rapid river.

I have already remarked that the caves of the North of England are commonly found in calcareous strata. This circumstance may be traced to natural causes; for the rain water descends with great ease through the vertical fissures of these rocks; which generally rest upon a base of gray schist, and in some places on a soft argillaceous substance of a laminated texture. This base is not uniformly flat: for it swells occasionally into lumps or hillocks; some of which appear above the surrounding limestone. Such of these hillocks as were originally situated under one, or a number of subterranean brooks formed in the calcareous strata, have been washed away long ago; and the caverns, which remain at present, shew the extent and form of these

demolished eminences. The recesses, thus produced, frequently contain pools of water; and if the presence of a grotto be necessary to a reciprocating fountain, perhaps few places are more likely to produce one, than the neighbourhood of Giggleswick. For the country abounds with caves, and also with subterranean brooks; one of which is heard very distinctly through the rocks which cover it, at a place where it sounds like a stream falling into an extensive chamber.

Having now finished my remarks on reciprocating fountains, I have only to recommend them to your attention. Should the essay appear to deserve the notice of your Literary and Philosophical Society, your kindness in presenting it to that learned body, will confer an additional favour upon

Your's, &c.

JOHN GOUGH.

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Nov. 1., 1811.)

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has any merit, is recommended by greater simplicity and economy, than those which have hitherto been applied to the same purpose. While it possesses these advantages, I am not aware that it is liable to objection from any sources of inaccuracy, that do not equally exist in all other eudiometers.

In its construction, it most nearly resembles, and indeed was originally suggested by, one which was invented, several years ago, by Professor Hope of Edinburgh. His apparatus consists of a tube sealed at one end, which holds precisely a cubic inch, and is

accurately graduated into 100 equal parts. This tube is fitted by grinding into the neck of a bottle, capable of holding two or three ounce measures of water, and having, near the bottom, another opening or neck, which is occasionally closed by a glass stopper. The bottle being filled with the eudiometric liquid, the tube containing the gas under examination is next put into its place; and on inverting the apparatus, the gas ascends into the bottle, where it is briskly agitated in contact with the liquor. An absorption takes place; and, to supply this, the stopper is taken out under water, which rushes into the bottle. The agitation, and opening of the stopper, are renewed alternately till no farther diminution is produced in the gas.

To this instrument, though very simple and ingenious, there are several objections: For 1st. by the absorption of part of the gas, the remainder becomes of less density, and is, therefore, less easily taken up by the liquid. 2dly. By the repeated admission of water, the eudiometric liquor becomes much weaker towards the close of the process, when its unimpaired strength is most wanted. 3rdly. If any defect exists in the joints of the vessel,

the external air rushes into the instrument to supply the vacuum.

All these objections, it occurred to me, after using the apparatus two or three times, might be obviated by substituting a bottle of caoutchouc or elastic gum, the sides of which, by collapsing as the absorption goes on, must place the included gas under an uniform degree of pressure during the whole experiment.* As a neck to the elastic bottle, for

* It would be unjust to Mr. Pepys, who has benefited chemical science by the invention of a variety of useful apparatus, not to state that he published the first account of an instrument, in which a bottle of elastic gum is used for containing the eudiometric liquid. (Phil. Trans. 1807.) As in his apparatus, however, the liquid is injected from the elastic bottle into the graduated tube, no contrivance was necessary for facilitating the return of the gas from the former into the latter; and his eudiometer, therefore, is adapted only for those liquids, which, like the solution of nitrous gas in sulphate of iron, act by a very moderate degree of agitation. The liquid, which I prefer, on account of the greater cheapness and facility of making it, is prepared by boiling a little quicklime, sulphur, and water, together in a Florence flask, decanting the clear fluid, and shaking it strongly in a bottle about three-fourths filled with it. To effect the absorption of oxygen gas by this liquid, especially towards the last, when it bears a small proportion to any other gas with which it is mixed, brisk and long continued agitation is necessary.

the purpose of receiving a graduated tube not differing from that of Dr. Hope, I employ a piece of tube of about $\frac{1}{2}$ an inch diameter, and about one inch long. Into one end of this, the graduated tube is accurately fitted by grinding; and the other end is made somewhat funnel-shaped as shewn by Plate VI. fig. 3. *b*. The outer surface of the wider tube being previously ground, to destroy its smoothness, the neck of the elastic bottle is firmly tied upon it, care being taken to bring the folds of string so low, that no space may be left for the lodgment of air between the bottle and the tube.

The apparatus is used in a similar way to that of Dr. Hope, the gas being measured from time to time to ascertain when the absorption ceases. The only difficulty, which is likely to be experienced, and which a little practice will overcome, is to return the whole of the gas from the bottle into the tube. Before measuring the residuary gas, it is proper to remove the graduated tube from its attachment, either under water or mercury; for otherwise the elasticity of the sides of the bottle increases a little its apparent quantity.

In most cases, the graduated tube may be cylindrical as shewn by fig. 5; but when

the residue of gas is expected to be very small, I employ a tube the sealed end of which is drawn out to a narrower diameter, so as to admit of more minute divisions (see fig. 6.) On the contrary, when only a small portion of gas is expected to be absorbed, the tube may be narrowest at the open end.

To satisfy myself of the adequacy of this instrument to its purpose, I compared the analysis of artificial mixtures of oxygen and nitrogen gases, by its means, with that effected by nitrous gas used in Mr. Dalton's mode; by phosphorus; and by detonation with hydrogen. The results, in order to avoid all bias in favour of any of the processes, were registered by Mr. H. Creighton, (to whom I am indebted for the annexed drawing) and when compared after the experiments were finished, they did not differ from each other more than $\frac{1}{100}$ of the whole mixture.

In graduating tubes for eudiometry or any other purpose, I have long been in the habit of using a contrivance, which renders the operation greatly quicker, and insures perfect accuracy. It consists of a tube (Plate VI. fig. 7.) open at both ends, and not more than .08 of an inch in diameter. This is carefully divided into equal parts, which may be en-

tirely arbitrary; but those, which I employ, are each ten grains of mercury at 60° Faht. the whole tube containing 100 grains. It is some trouble to divide this tube; but, when once prepared, any number may, by its means, be easily graduated. The successive portions of mercury, used in dividing wider tubes, are measured by this, into which they are drawn, either by plunging it into a jar filled to sufficient height with that fluid, or by the action of the mouth.

The two figures in the plate, which remain to be described (fig. 1. and 2.) represent an apparatus, which I have found extremely useful for submitting various gases to the long continued action of electricity. The platina wires, for conveying the electric fluid, are inclosed in two short pieces of almost capillary tube *b c*, which are sealed round them, and then ground away so as to expose merely the points at *d d*. These tubes are hermetically sealed into the small globe at *b c*, so that the points of the wires may be at a proper striking distance. The vessel may be filled with gas over mercury, and closed by the stopper *g*, fig. 2, or the elongated stopper *c*, fig. 1. But if it is desirable entirely to

exclude mercury, some small globules of which always remain in the globe when filled over that fluid, a metal cap may be cemented upon the neck of the vessel (fig. 1.) which, after exhausting it by the air pump, may be filled with gas from a receiver furnished with a proper stop-cock. An apparatus of this kind was used in the experiments on muriatic and oxymuriatic acids, which I have described in the Philosophical Transactions for 1812; and may be advantageously applied to other purposes.

A MEMOIR

ON THE

URIC ACID,

BY WILLIAM HENRY, M. D. F. R. S. &c. *

(Read Nov. 29, 1814.)

SECT. I.

History of Discoveries respecting the Uric Acid.

THOUGH the properties of the Uric Acid have not been well understood, until the last thirty-five years, yet it appears from the writings of some of the earlier chemists, that they had made very near approaches to the discovery of its real nature.

Van Helmont, by the destructive distillation of an urinary calculus, obtained what he calls a fœtid spirit, a yellow crystalline mass, and an oily product, all resembling the substances which may be obtained by a similar

* The principal part of this essay was published in my inaugural dissertation at Edinburgh in 1807; but having since repeated most of the experiments, I have corrected some of the results, and added those of new ones.

treatment of the dry extract of urine.(a) Hoffman, having placed a fragment of a stone from the kidney upon a red hot coal, found that it emitted a smell of volatile alkali, and left a portion of charcoal, which was perfectly tasteless. He ascertained, also, that the same substance was very sparingly soluble in water, and not at all in sulphuric or muriatic acids at common temperatures; but that hot nitric acid acted upon it, and gave a solution, which was not precipitated by carbonate of pot-ash. Hence he concluded, with reason, that concretions of this kind do not consist of calcareous earth.(b) Slare, in order to shew that the stone of the bladder is not tartar, submitted it to destructive distillation, and obtained oil, volatile alkali, and a brown and bitter salt. A coal remained, which was nearly consumed by burning it with free access of air.(c) Dr. Hales, in addition to the same condensible products, collected a large quantity of permanent gas, amounting to 516 cubical inches, from a quarter of a cubical inch of the stone. He denies the power of dissolving calculus, not only to the sulphuric acid, but to alkaline salts; evidently in con-

(a) De Lithiasi, cap. v. § 9. Amst. 1648.

(b) Obs. Phys. et Chem. lib. ii. obs. 25. Genev. 1748.

(c) Lowthorp's Abridgment of the Phil. Trans. iii. 179.

sequence of his having employed the mild, instead of the caustic alkalis. Nitric acid, however, he found to act on the stone with an effervescence, which he shews to be owing to the formation of a permanent gas.*(d)*

No further examination appears to have been made of this substance for nearly half a century; for it was not till the year 1776, that Scheele published the excellent essay, which contains the first accurate history of its chemical properties.*(e)* With Hoffman and Hales, he found the matter of calculus * to be soluble in nitric acid; and he added the observation, that by evaporating the solution to dryness, a red mass is obtained, which imparts its peculiar colour to the skin and other animal substances. This property is of importance, inasmuch as it distinguishes the body in question from all others. Scheele, also, first pointed out its title to be ranked among acids, in consequence of its reddening the infusion of turnsole. He determined, moreover, that the acid, which he had found

(d) Hales's Hæmastatics 1727, p. 190.

(e) Scheele's Essays, Essay IX.

* It is necessary to remark that this applies to one species of calculus only; and that there are several kinds, differing not only in external appearance but in chemical composition.

in urinary calculi, is not merely a product of disease, but is constantly present in the urine, even in its most healthy condition.* Sometimes, from urine which had been voided a few hours, he observed it to be deposited in small crystals; or, when the separation did not take place spontaneously, he found that it might be produced by evaporating the urine to one fourth, or one third of its bulk. Bergman confirmed the experiments of Scheele,(*f*) and their united authority was deemed sufficient by the framers of the French nomenclature, (*g*) to entitle the newly discovered substance to a distinct place among the acids. Having been originally obtained from the stone of the bladder, they derived the name of *Lithic Acid*, from the Greek word λιθος, lapis.

* The uric acid was till lately supposed to be peculiar to the urine of the human species. (Ann. de Chim. xvi. 166.) Mr. Brande, however, has found it in the urine of the camel, but not in that of other animals that feed chiefly on vegetables. (Phil. Trans. 1806, p. 373.) And Dr. Wollaston has found that it forms a considerable part of the urine of birds, which is voided along with their dung, especially of such as are carnivorous. (Phil. Trans. 1810. p. 229.)

(*f*) Act. Stockh. an. 1776, Opusc. Phys. et Chem. iv. 387.

(*g*) Methode de Nomenclature, 1787.

The next series of experiments on the lithic acid, were published by Mr. Higgins of Dublin; *(h)* and these were soon afterwards followed by the researches of the late Dr. Austin. *(i)* The mode of investigation, adopted by both these philosophers, was chiefly that of destructive distillation. Little, therefore, was added to our knowledge of the subject, except the discovery, by Mr. Higgins, that nitrate of ammonia is produced by the action of nitric acid on this variety of calculus; and a more accurate examination, by Dr. Austin, of the permanently elastic fluids.

In the year 1793, Mr. Murray Forbes, in an ingenious treatise on Gravel and Gout, *(k)* pointed out a mode of separating the lithic acid from urine, which has the merit of great simplicity and efficacy. It consists in adding diluted sulphuric acid, to recently voided urine (in the proportion of about 20 drops of the former, to half a pint of the latter,) and in allowing the mixture to stand about 24 hours. At the expiration of that time, small

(h) Compar. View of the Phlogistic & Antiphlog. Theories, p. 283.

(i) Treatise on the Origin and component parts of the Stone of the urinary bladder, 1791.

(k) Published in 8vo. at London, 1793.

crystals are found adhering to the sides of the vessel, which may be collected and purified by washing them with cold water. The invention of this process, since described by Dr. Egan in the Transactions of the Irish Academy, (*l*) is, indeed, to be attributed to Link, who first made it known in his Dissertation, published at Gottingen, in 1788. (*m*)

Beside the peculiar variety of calculus which consists chiefly of uric acid, Dr. Wollaston, in the Philosophical Transactions for 1797, described several other well characterized species; and proved that the concretions, which are found in the joints of gouty persons, consist of lithic acid united with soda. Dr. Pearson, in the following year, was led by a long and laborious investigation of the properties of the lithic acid, to a conclusion respecting its nature different from that of Scheele and Bergman; for its properties, he conceived, agree better with those of an oxide than of an acid, and he proposed, therefore, to call it the *uric oxide*. (*n*) His memoir incited Fourcroy and Vanquelin to repeat and extend the experiments of Scheele, whose

(*l*) Vol. x. p. 256.

(*m*) H. F. Link Commentatio de Analysi Urinæ et Origine Calculi.

(*n*) Phil. Trans. 1798.

original conclusions they fully verified. (*o*) They concede, however, to Dr. Pearson the propriety of using the specific name *uric;* and this term has since been generally received by chemical philosophers. My own experiments, it will presently appear, fully confirm the propriety of ranking it in the class of acids.

SECT. II.

On the Chemical Properties of the Uric Acid.

THE following account of the properties of the Uric Acid, is to be understood as applicable to it in a pure state. To obtain it in sufficient quantity, I have generally had recourse to that variety of urinary calculus, which is chiefly composed of uric acid. Concretions of this sort may readily be distinguished by their external characters. They are of various sizes, from that of a horsebean to that of a large egg. Their shape is generally a flattened oval; and, when broken or divided by a saw, they exhibit generally a radiated structure, and have a central nucleus of more compact texture and greater hardness than the rest of the stone. Their colour

(*o*) Ann. de Chim. xxvii. 225. Fourcroy's Systeme, tom. v. 4to. p. 515.

is various, from pale straw yellow to deep brown, sometimes with an intermixture of red; and the divided surfaces bear considerable resemblance to wood. To separate the uric acid from the other substances with which it is mixed, the calculus, finely powdered, is to be dissolved in a heated solution of pure potash. The solution is to be poured into a quantity of diluted muriatic acid, which is more than sufficient to saturate the alcali; and the precipitate is to be repeatedly washed with a large quantity of distilled water. In order to remove any adhering portion of muriatic acid, a little carbonate of ammonia, may also be added to the first washings. After edulcoration, it may be dried in a temperature not exceeding 212° Faht.

1. In this state, the uric acid has the form of white shining plates, somewhat resembling those of the acid of borax, but considerably smaller. It is perfectly soft to the touch, and entirely destitute of taste and smell.

2. When added in powder to the infusion of litmus, it changes the blue colour of that liquid to red, but less distinctly than the mineral and most of the vegetable acids.

3. Four ounce measures of boiling distilled water take up about 1.4 grain, and of this about half a grain separates again on cooling.

According to Dr. Pearson, the acid is soluble in only 800 times its weight of water; and Scheele states the quantity required at still less, viz. 300 parts. It is to be observed, however, that Scheele employed, for his experiments, only the pulverized calculus, and not the purified acid.

4. The watery solution reddens the infusion of litmus, but produces no change on the solution of alkaline carbonates.

5. When a small portion of the dry acid is heated on a bit of window glass with a few drops of nitric acid, and the mixture is evaporated to dryness, the residuum has a beautiful red colour. The addition of a few drops of water greatly increases its intensity; and occasions it to resemble that of carmine. This colour is communicated to the skin, to wood, and to other animal and vegetable substances. It is also soluble in water, and the solution has the hue of an acidulated infusion of rose leaves; but soon loses it and becomes limpid, even when secured from the access of air. The colour is destroyed by all acids, and by pure alkalis; and is not restored again by any chemical agent which I have employed with this view. Fourcroy (*p*) ascribes this charac-

(*p*) Systéme, 4to. V. 516.

teristic property (of affording a red colour with nitric acid) to the admixture of urea, and denies it to the pure uric acid. I have satisfied myself, however, that it belongs even most remarkably to the purified acid, and that it cannot by any process be obtained from urea.

6. The watery solution of uric acid does not produce any change in the solutions of earths or metals in acids.

7. The dry acid is not at all acted upon by the solutions of the alkaline carbonates, or sub-carbonates. Even digestion with them, for several hours, occasions no greater loss of weight, than would be produced by a quantity of water equal to that of the solution. This fact I determined by repeated experiments, both on account of its influence on medical practice; and because, though testified by every preceding writer, it has lately been denied by Dr. Egan. (*q*) It suggests the necessity of administering alkalis in a pure state, whenever they are given with the view of dissolving a stone, which is already formed in any of the urinary passages.

8. The watery solution of uric acid does not decompose soap; but when the dry acid

(*q*) Irish Transactions x. 289.

is digested with a solution of that substance, the oil is detached, and a liquid results, which bears much resemblance to an emulsion. Ten grains of uric acid, digested with 30 grains of soap, and four ounce measures of distilled water, at a temperature of 180° Faht., were dissolved, except a small portion, estimated at most at half a grain. It appears, therefore, that soap may be expected to exert a solvent action on those uric acid concretions, which are lodged in the urinary passages.

9. The compounds of sulphur and sulphuretted hydrogen with alkalis are decomposed, when heated with uric acid.

10. The uric acid is not dissolved, when digested with a solution of prussiate of potash.

11. It is not acted upon by any acid, except by those which, at the same time, effect its decomposition, viz. the sulphuric, nitric, and oxymuriatic, the agency of which will be described, after detailing the properties that belong to the acid in its entire state.

12. The uric acid is rapidly dissolved by heated solutions of pure potash and pure soda, but less readily by that of ammonia. An ounce measure of liquid potash, of the specific gravity 1108, dissolves about 60 grains of the acid. The solution has a strongly

alkaline taste, and is decomposed by all acids. Even the carbonic acid and the alkaline carbonates occasion a white precipitate from it. The nature of this precipitate differs, however, according to the circumstances under which it has taken place. If the alkaline solution be poured into diluted muriatic, sulphuric, or any other strong acid; or if these acids be employed in any way, provided their quantity exceeds what is necessary to saturate the alkali, uric acid is precipitated in a pure state. But if the precipitating acid, on the contrary, be gradually added to the alkaline solution, and in a quantity insufficient for its saturation, the precipitate is, either wholly or in part, an insoluble compound of uric acid and alkali. This saturated compound of uric acid and alkali is alone thrown down by solution of carbonate of ammonia, and by carbonic acid.* The alkaline solution may, therefore, be regarded as consisting of a neutral compound of uric acid and alkali, dissolved by an excess of the latter substance. To obtain the saturated compound, we may either directly combine the uric acid with the alkali,

* This fact escaped the observation of Scheele, whose sagacity and accuracy on most occasions are singularly conspicuous. See his 9th Essay, § 4.

in such proportions as mutually saturate each other; or we may adopt the easier method of forming a solution of uric acid, by an excess of alkali, and then precipitating by carbonate of ammonia, and edulcorating the sediment. The latter process answers best, when we employ potash or soda; but to obtain saturated compounds of uric acid with ammonia, baryta, strontita, lime, magnesia, or alumine, I have generally had recourse to the former method. In whatever mode these compounds are prepared, they are termed Urates.

SECT. III.

Urates.

THOUGH I have examined the properties of each individual urate with great attention, yet they do not appear to me sufficiently important, to entitle each of them to a separate history. It will, therefore, be sufficient to state those properties, which are common to the whole of this genus of salts.

1. The urates are all perfectly insipid, and, when moist, are scarcely distinguishable from the uric acid itself. In the act of drying, however, they shrink somewhat like alumine, and form hard masses.

2. They are all permanent, or undergo no change, by exposure to the atmosphere.

3. Though more soluble than the uric acid itself, yet they are universally difficult of solution, even by hot water. Of urate of potash, an ounce of boiling water takes up about a grain. This is the most soluble; and the rest succeed it in the following order, urates of soda, baryta, strontita, lime, ammonia, magnesia, and alumine.

4. They are decomposed by a red heat, and after being burnt with access of air, the base remains in the state of a carbonate, excepting when we employ the urate of ammonia.* After being thus decomposed, the quantity of alkali, which has saturated the acid, proves to be extremely small. The urates of potash and soda, after the destruction of their acid in this way, leave only about one eighth their weight of the respective subcarbonates of those alkalis. Also, ten grains of uric acid, dissolved by potash or soda, and precipitated by carbonate of ammonia, give from 9 to 10 grains of dry urate. This fact shews, 1st. That the uric acid

* Mr. Forbes, who composed the urates of magnesia and alumine, and investigated their properties, remarks that after evaporation to dryness, they emit volatile alkali at a degree of heat not very considerable. (On Gout, &c. p. 15.)

contains a small portion of water, which it loses either wholly or in part by combining with the alkalis; and 2dly. That the quantity of alkali required for neutralization is excessively small.

5. When to a watery solution of any of the urates, we add the sulphuric, nitric, muriatic, or any other acid, except the prussic or carbonic, the uric acid is precipitated, from the more soluble urates immediately, and from the less soluble after some interval of time.

6. Solutions of the alkaline urates are decomposed by the muriates, nitrates, and acetates of baryta, strontita, lime, magnesia, and alumina, but least readily by those of magnesia.

7. They are also precipitated by the solutions of all metals, except that of gold. The precipitate by solutions of iron has a tinge of red, and that by solutions of copper a greenish hue; but all the other precipitates are white, and extremely difficult of solution.

8. The saturated urates are mostly soluble by an excess of their respective alkaline or earthy bases. Those of ammonia, magnesia, and alumine, are exceptions.

From a consideration of the properties which have been already described, as belonging to the peculiar substance, which

forms the chief ingredient of urinary calculi, there can be little room for doubt about referring it to the class of acids.

1st. Because it reddens the infusion of litmus. It must be acknowledged that Dr. Pearson has given a contrary statement; (*r*) but his result was probably obtained, by employing a substance which had been precipitated by a deficiency of acid. In that case, he must necessarily have operated not on uric acid, but on a saturated urate so closely resembling the acid, as not to be distinguishable by external properties. It may be alledged, indeed, that the uric acid, which I employed, might retain a portion of the muriatic acid used for its precipitation; but this is not at all probable, since it was well edulcorated by carbonate of ammonia. Besides, the powdered stone itself produces the same effect; and certainly not from any mixture of super-phosphate of lime, for which, relying on the authority of Brugnatelli, (*s*) I have in vain sought in several specimens of uric calculi.

2dly. Because it decomposes, as Dr. Pear-

(*r*) Phil. Trans. 1798.

(*s*) Ann. de Chim. xxv. 55.

son admits, the compounds of alkalis with sulphur, and with sulphuretted hydrogen.

3dly. Because it detaches the oil from soap. That Dr. Pearson did not obtain this result, may be ascribed to his having used either a saturated urate, or an insufficient quantity of uric acid; for it is well known that even the stronger acids, added in too small a proportion to solution of soap, scarcely effect any change in it. To produce this change with uric acid, it is essential that it should be added in powder and in due quantity, and that its action be assisted by heat.

4thly. An unequivocal test of the acidity of this substance is, that it forms with the alkalis and earths, chemical compounds, in which the qualities, that belonged to them when separate, are no longer apparent. To the evidence of all these properties, it cannot be sufficient to object the want of sourness to the taste, a quality which is equally deficient in the prussic acid. We may safely, therefore, consider the body in question as entitled to be ranked in the same class of chemical compounds; but its acid power is extremely feeble, as is proved by the very small proportion of alkali which it is capable of neutralizing.

SECT. IV.

Decomposition of the Uric Acid by other Acids.

On this subject I have no additions to make to the facts which have been stated by other chemists, whose testimony, so far as I have examined it, I have found to be perfectly correct.

1. Concentrated sulphuric acid and uric acid, when heated together, are mutually decomposed; and sulphureous and carbonic acid gases are obtained. (*t*)

2. The mutual destruction of the nitric and uric acids, was first determined by Bergman, who observed that the red stain, left after heating the two acids together, was itself scarcely acid. The action of these acids on each other has since been farther investigated by Mr. Higgins, (*u*) and Dr. Pearson. (*x*) The latter chemist, by repeatedly distilling nitric acid, from the same portion of uric acid, effected its entire decomposition. The nitric acid, yielding its oxygen to the carbon of the animal acid, formed

(*t*) Scheele, Essay IX. § 1.

(*u*) On Phlogiston, p. 299.

(*x*) Phil. Trans. 1798.

carbonic acid; while its nitrogen, with the hydrogen of the uric acid, formed carbonic acid; and this, uniting with a portion of undecomposed nitric acid, composed nitrate of ammonia.

3. The oxymuriatic acid, according to the same chemist, also generates ammonia with uric acid; and the volatile alkali remains combined with muriatic acid, the muriate of ammonia being the only substance which he obtained. Fourcroy, however, asserts that in addition to this product, he obtained acidulous oxalate of ammonia, and the muriatic and malic acids in an uncombined state: and Brugnatelli observed the formation of oxalic acid.

SECT. V.

Destructive Distillation of the Uric Acid.

THE distillation of the uric acid *per se*, with a view both to the condensible and permanently elastic products, has been performed by Scheele, by Mr. Higgins, by Dr. Austin, and by Dr. Pearson, whose statements do not essentially differ from each other. The results are carburetted hydrogen and carbonic acid gases; prussic acid; carbonate of ammonia; and an acid sublimate of peculiar properties. It is also commonly stated that

a portion of the uric acid is volatilized unaltered; but this I have never been able to observe, and I believe that volatility is not one of its properties. Using a succession of receivers, and taking the products at various periods, I have remarked them to be formed in the following order, 1st. A very minute portion of water, not exceeding a drop or two from 100 grains of the acid, impregnated with carbonate of ammonia; then concrete carbonate of ammonia; next prussic acid; and afterward the peculiar sublimate of Scheele, in the proportion of about one fourth the calculus employed. In the retort there remains about $\frac{1}{6}$ the weight of charcoal.

The nature of this sublimate not having been sufficiently examined, I investigated its properties with considerable attention. Scheele believed it to resemble the succinic acid; but Dr. Pearson thought that its qualities are rather analogous to those of benzoic acid. The experiments, which I have made, lead me to infer that it contains neither of those acids; but that it is composed of ammonia united with an acid *sui generis*. Its properties are the following:

1. It has a yellow colour, a cooling bitter taste not mixed with that of any acid, but strongly flavoured with an animal empyreuma.

2. It readily dissolves in water, even at common temperatures, and in alcohol. It is soluble, also, in alkaline solutions, but is not precipitated by acids; thus evincing a marked difference from the uric acid and its compounds.

3. It is volatile, and, by repeated sublimations, is greatly improved in freedom from colour.

4. Its watery solution reddens the infusion of litmus, but a single drop of solution of ammonia destroys this property in a considerable quantity of the solution, thus proving that the acid is only slightly in excess.

5. When the watery solution of the sublimate is slowly evaporated, it shoots into crystals. The shape of these is not well defined, owing to their mixture with a portion of resinous matter, resulting from the oxygenizement of an essential oil, which the sublimate always contains. Repeated crystallizations do not entirely purify the salt, though they render it much whiter, nor do they deprive it of its excess of acid.

6. When the crystals are added to solution of pure potash, they emit a smell of ammonia.

7. They do not, after being evaporated to

dryness in mixture with nitric acid, give a red stain as the uric acid does when similarly treated.

8. The watery solution does not, like the alkaline urates, decompose neutral salts with earthy bases.

9. It has no action on salts with base of copper, iron, gold, platina, tin, or mercury. It differs, therefore, from succinate of ammonia, which precipitates solutions of iron and tin; and from the alkaline urates, which decompose all metallic salts, except that of gold. The solution of the sublimate agrees, however, with succinate of ammonia, in throwing down, from nitrates of silver and mercury and from acetite of lead, a white precipitate, which is soluble by an excess of nitric or acetic acids.

10. It differs from benzoate of ammonia, in not being precipitated by muriatic acid, which instantly separates benzoic acid from the latter salt. The precipitates, also, from metallic solutions by benzoate of ammonia, are not re-dissolved by nitric or acetic acid.

These properties sufficiently shew that the acid ingredient of the sublimate is not either the succinic or benzoic, but one distinguished by a peculiar set of properties.

Dr. Austin has proved that the sublimate itself may be decomposed by the action of heat, and may be resolved into ammonia, azotic gas, and prussic acid. He has ascertained, also, that when heated with nitric acid it affords carbonic acid and nitrogen gases. As to the nature of its components, it agrees in general with the uric acid, from the disunion of whose elements, and their re-combination in a new manner, it undoubtedly results. Both substances contain oxygen, hydrogen, carbon, and nitrogen, but in different proportions, which I am not at present able to assign. It is only, indeed, of late, that the improved instruments and methods of analysis, invented by Gay Lussac and Thenard,* have enabled us to determine minutely the composition of animal and vegetable substances; and I have not yet been able to furnish myself with the apparatus, which is necessary to the successful prosecution of this branch of the enquiry.

* Recherches Physico-Chimiques, Tom. ii.

A DEMONSTRATION
OF
LAWSON'S
GEOMETRICAL THEOREMS:

BY THE LATE REV. CHARLES WILDBORE;

Communicated by Mr. Mabbott to Mr. Ewart, and by him to the Society.

(Presented January 10, 1812.)

To Peter Ewart, Esq.

DEAR SIR,

I REQUEST you to present the inclosed manuscript to the Literary and Philosophical Society. It contains solutions, by that very able mathematician, the late Rev. Charles Wildbore, to all the sixty theorems in the well known pamphlet entitled, "A Dissertation on the Geometrical Analysis of the Ancients, with a collection of theorems and problems, without solutions, for the exercise of young students; 1774." These theorems have all been elegantly demonstrated before, in Leyburn's Mathematical Repository. But I esteem the following train of solutions to be a very curious specimen of investigation, and a proper exemplification of the method, which the ingenious author of the theorems recom-

mends, of inventing and deriving one geometrical property from another, to an almost endless variety. I have sent you herewith a copy of the Pamphlet containing the theorems, as it may be thought necessary they should accompany the solutions. It is well understood that the author of the "Dissertation" was the late Rev. John Lawson, B. D.

I remain your's truly,

J. MABBOTT.

Manchester, Jan. 8, 1812.

Mr. Wildbore's Demonstration of Lawson's Theorems, &c.

The author at page 18, of his Pamphlet on the Analysis of the Antients, very justly observes, that in the resolution of problems there is often need of a previous preparation, a kind of mental contrivance and construction, in order to form a connexion between the *data* and *quæsita*. And I would not have it concealed that herein consists the great difficulty of this branch of science. Nor do I know any advice so proper to give the admirers of these rational amusements, as to endeavour to attain a facility of investigating or invent-

ing one geometrical property from another. It is for their assistance herein, and not from any supposed excellence of the solutions above (though most of them are different from those of the original authors themselves,) that I have taken the trouble to run through, and investigate the 60 Theorems. I believe I may safely say that any person that will take the trouble to follow me herein, will find it worth his while, and may in a short time, from hence find out many times this number of theorems of like nature, and equally curious with these. And as this may possibly fall into the hands of some more learned readers, I would wish them to think, whether or no this may not possibly be a specimen of a method of investigation similar to that of the Ancients, which has been a *desideratum* ever since the Saracens burnt the library at Alexandria in Egypt.

Diagram I.—*(Plate IX.)*

Draw ED the perpendicular of the isosceles triangle BEC, and AH through the vertex, parallel to the base; from any point A in which, draw lines through the extremes of the base and perpendicular, viz. AC, AB, AD; and through D, the extreme of the perpendicular, a line *ad libitum*, cutting AB in F,

AC in G, AH in H; and bisect DH in n. Then because the triangles DGC, AGH are similar, DC : AH : : DG : GH; and because BFD, AFH are similar, BD = DC : AH : : FD : FH. Therefore by equality of ratio DG : GH : : FD : FH. *Which is* 3 *the third Proposition.*

Join EF, EG cutting BC in k and l, through G draw Gi ‖ to EH cutting ED in o and EF in i; then by reason of the parallel lines, AH : EH : : BD : Dk : : DC : Dl, and because BD = DC, ∴ Dk = Dl; consequently ED bisects the ∠ FEG, and EF : EG : : FD : DG. *Which is the fifth* 5 *Proposition.*

Let fall Fm perp. to ED produced; then

LAWSON'S

GEOMETRICAL THEOREMS.

PROP. I.

IF a right line AB be bisected in E, and two points C and D taken therein such that AC : CB : : AD : DB; then I say the rectangle D C E = the rectangle ACB.

The converse of this proposition is also true, which is this.

If a right line A B be bisected in E, and two points C and D taken therein such that DCE = ACB; then I say AC : CB : : AD : DB.

PROP. II. If in A B the diameter of a circle two points

because FD : DG : : FH : GH, therefore Dm : Do : : Em : Eo, and FE : EG : : FD : DG : : FE : Ei. Therefore if in any line Emo, be taken two points D E such, that Dm : Do : : Em : Eo, and m F, o G be drawn perp. to Em, and through the point D, be drawn any line to meet mF, oG in F and G, and EF, EG be joined; then FE : EG : : FD : DG, and FE : EG : : FE : Ei. *Which is the sixth Proposition.*

6

Since Gi is bisected by the perp. Do, ∴ Di = DG; and because ∠ DEH is right, ∴ En is equal to nH, and parallel to iD; because iG is parallel to EH, therefore the lines FG, FH are similarly divided in the

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C and D be assumed such that AC : CB : : AD : DB, and from D an indefinite perpendicular to the same diameter as L D be erected, and through C any line be drawn to cut the same in E, and the circle in F and G; I say FC : CG : : FE : EG.

The converse of this proposition is also true, which is this.

If any right line as L D be drawn perpendicular to the diameter A B of any circle and meets the same in D, and if from a point in the same diameter, as C, any line be drawn to meet the same perpendicular in E, and the circle in F and G, so that FC : CG : : FE : EG; I say that AC : CB : : AD : DB.

Prop. III. Let there be a triangle A B C, whose base B C is bisected in D, and through the vertex A a line /. E

points D and n, or Fn : FH : : FD : FG. 1 *Which is the first Proposition.*

On the centre n, describe the semi-circle DEH, join FE, cutting the circle again in h, erect G g perp. to FH, cutting FE in g, and the circle in T; then since FG : FD : : FH : Fn, therefore by division, FG : FD : : GH : Dn = Hn, and FG : GH : : DG : Gn, or DG . GH = FG . Gn, but DG . GH = TG^2, therefore FG . Gn = TG^2; consequently the points F, T, n are in a semi-circle, and FT a tangent to the circle DEH. *Which is the* 15 *fifteenth Proposition.*

Produce EG, hG till they cut the circle

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drawn parallel to B C, and any line drawn through D to meet A B, A C, A B in F, G, H; then I say GD : DF : : GH : HF.

Prop. IV. If in AB the diameter of a circle two points C and D be taken such that AC : CB : : AD : DB, and through the point D any line be drawn to meet the circle in E and F, and CE, CF be joined; then I say EC : CF : : ED : DF.

Prop. V. If the base BC of a triangle be bisected in D, and through the vertex A a parallel thereto be drawn, and from D a perpendicular to BC be drawn to meet the parallel in E, and through D any line be drawn to meet AB, AC in F and G, and EF, EG be joined; then I say EF : EG : : FD : DG.

Prop. VI. If in the line AB be taken two points C and

again in h′, E′, and TG to T′; join FT′, which must be equal to FT, and join EE′, cutting FH in f; then because the angle hEh′ = hE′h′, and EhE′ = Eh′E′, therefore h′hE′ = EE′h, hE′ = h′E, h′E′ = hE; also because hGE = h′GE′, ∴ Gh = Gh′, GE = GE′; but GT = GT′, therefore EE′, as also hh′, is parallel to TT′; and because FhE is a right line, ∴ Fh′E′ is a right line and the triangles TGE, T′GE′ equal and similar, and therefore gEG, g′E′G are so; consequently the angle EGg = E′Gg′ = hGg, and gE : hg : : EG : Gh = Gh′ : : Ef : hq : : EE′ : hh′ : : FE : FH. Hence if in DH the diameter of a circle, two

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D such that AC : CB : : AD : DB, and AE, BF be drawn perpendicular to AB, and through the point C be drawn any line to meet AE, BF in G and H, and DG, DH be joined; then I say that DG : DH : : GC : CH.

Prop. VII. If in the diameter of a circle AB be taken any point C, and CDE be drawn meeting the circle in D and E, and DF be perpendicular to AB meeting it in F, and the circle again in G, and EG be joined meeting AB in H; I say that AC : CB : : AH : HB.

Also, as the converse, that if in the diameter AB two points be taken as C and H such that AC : CB : : AH : HB, and from the points C and H two lines CE, HE be inflected to any point of the circumference E meeting the same again in D and G; when DG is drawn, it will be perpendicular to AB.

points F and G be assumed, such that FD : FH : : DG : GH, and from G an indefinite perp. be erected, and through F any line be drawn to cut the same in g, and the circle in h and E, then Fh : FE : : hg : gE. *Which is the second Proposition.* 2

Also EF : Fh′ : : EG : Gh′ = Gh. *Which is the fourth Proposition.* 4

Also if in the diameter of a circle, any point F be taken, and FhE be drawn meeting the circle in h and E, and h q be perpendicular to DH meeting it in q, and the circle again in h′ and Eh′ be joined meeting DH in G; then FD : FH : : DG : GH. *Which is the seventh Proposition.* 7

Prop. VIII. If in the diameter of a circle AB two points C and H be taken such that AC : CB : : AH : HB, and from the points C and H be inflected to any point of the circumference E two lines CE, HE meeting the same again in D and G; I say that EC : CD : : EH : HG.

Prop. IX. If in AB the diameter of a circle be taken any point C, and CD be drawn meeting the circumference in D and E, and from the point D be drawn DF perpendicular to CD, which meets the diameter AB in F and the circumference in G, then I say that DC : CE : : DF : FG.

Prop. X. If in AB the diameter of a circle two points C and D be taken such that AC : CB : : AD : DB, and through the centre E a perpendicular to AB be drawn, and from C a line be drawn to meet the same in F, and if through D any line DG be drawn to meet the circle in G

Again, if in the diameter of a circle DH, two points F, G be taken such that FD : FH : : DG : GH, and from the points F and G, be inflected to any point of the circumference E, two lines FE, GE meeting the same again in h and h′.

8 Then Fh : FE : : Gh′ : EG. *Which is the eighth Proposition.*

Perpendicular to FE, draw Ee, cutting the diameter in c, and the circle in e ; then the angle EcF = FEf = FgG, and h′eE = h′E′E = FEf; ∴ h′ e is parallel to FH,

9 and Fh = Fh′ : FE : : Gh′ : EG : : ce : Ec. *Which is the ninth Proposition.*

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and H, and from the point G be drawn GK the same side of DG as F is of the diameter AB to make the angle DGK equal to the angle CFE, and let the line GK meet the circle in L and the line CF in M; then I say that GM : ML : : GD : DH.

Prop. XI. If from any point C in the diameter of a circle produced a perpendicular be raised and from any point D in the same a line be drawn to cut the circle in E and F; then I say the rectangle EDF is equal to the rectangle ACB together with the square of CD.

Prop. XII. If from any point C in the diameter of a circle produced a perpendicular be raised and thereon CD be taken whose square is equal the rectangle ACB, and CE be put equal CD, and from any point in DE as H a line be drawn to cut the circle in F and G; then I say twice the

Diagram II.

Perpendicular to n the center, or any other point of the Diameter DH, erect $n\varphi$, and make the $\angle \varphi Fn = FEG$; then $F\varphi n = GEe$; produce $F\varphi$ till it meets Ee in m; then $\varphi Fn = heh' = hnF$; therefore Fm is parallel to h e, and h'G : GE : : Fh : FE : : em : Em. *Which is Proposition tenth, part 1st.*

Also if through the point h, any line hL be drawn to the circle, and at G the $\angle gGM$ be made equal to LhE, the points M, G, g, h are in a circle, therefore the

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rectangle FHG is equal to the sum of the squares of HD and HE.

Prop. XIII. If in AB the diameter of a circle two points C and D be so taken that, C being without, and D either within or without the circle, the square of CD be equal to the rectangle ACB, and from C a perpendicular to AB erected, and any line drawn through D to cut the same in G and the circle in E and F; then I say the square of GD will be equal to the rectangle EGF.

The converse is also true, which is this.

If GC be perpendicular to AB the diameter of a circle and meets it without the circle in C, and if from G a line be drawn to cut the circle in E and F, and the diameter either within or without in D, and the square of GD be

angle hMg = the supplement of hGg, and consequently of hE′E; therefore hLE = hMg, Mg parallel to LE, and hM : ML : : hg : gE : : Gh′ : EG : : Fh : FE. *Which is Proposition tenth, part 2d.*

At F erect a perpendicular to FH, produce Hh till it meets it in ϑ, and join Dh; then the ∠ DhH being right, F, ϑ, h, D are in a circle; therefore Hh . Hϑ = FH . DH = FH² — FD . FH = Hϑ² — Hϑ . ϑh; ∴ Hϑ . ϑh = Hϑ² — FH² + FD . FH
11 = Fϑ² + FD . FH. *Which is the eleventh Proposition.*

If Fd′ = Fa = Fq = FT, then ϑa = Fq + Fϑ, ϑd′ = Fq — Fϑ, ϑa² + ϑd′² = 2Fq²

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equal to the rectangle EGF; then I say the square of CD will be equal to the rectangle ACB.

Prop. XIV. Things remaining as in the last proposition, if the perpendiculars Eg and FH be demitted; then I say that the rectangle gCH is equal to the square of CD.

Prop. XV. If from C any point in the diameter of a circle AB produced a tangent be drawn, and from the point of contact D a perpendicular to the diameter DE be demitted; then I say that AC : CB : : AE : EB.

Or conversely thus:

If in AB the diameter of a circle be taken two points C and E such that AC : CB : : AE : EB, and from E a perpendicular ED raised, and CD drawn; then I say CD touches the circle in D.

Or thus:

$+ 2Fd^2 = 2dq^2 = 2FD \cdot FH + 2Fd^2 = 2Hd \cdot dh$. Hence if from any point F in the diam. of a circle produced, a perpendicular be raised, and thereon Fd be taken whose square is equal to the rectangle DFH, and Fa be put equal to Fd and from any point in da as d a line be drawn to cut the circle in any two points as h and H, then twice the rectangle hdH is equal to the sum of the squares of dq and dq. *Which is the twelfth Proposition.* 12

If ds be drawn through q cutting the circle in r and s, then the rectangle $= rd \cdot ds = hd \cdot dH =$ (by the last) dq^2. *Which is the thirteenth Proposition.* 13

If in AB the diameter of a circle produced, a point C be taken, and therefrom a tangent as CD be drawn, and in the diameter a point E be taken such that AC : CB : : AE : EB; then I say ED being drawn will be perpendicular to the diameter AB.

Prop. XVI. Let AB be any chord in a circle and CD another cutting the former in E, CB being joined, from D draw DF parallel to CB to meet AB in F; I say that the rectangle AEF is equal to the square of DE.

Prop. XVII. If ABC be a triangle inscribed in a circle whose sides CA and CB are equal, and the rectangle CBD equal to the square of AB, and let AE be any line cutting CB in F and the circle again in E, and from E let a parallel to AB be drawn to meet CB in G; then I say that the rectangle CFG : BF^2 : : CG : BD.

fall the perpendicular sw, rv, then sw : Fq : : sr : sq : : (by the last) sq : ss : : Fq : Fw; therefore the rectangle vFw = Fq². *Which is the fourteenth Proposition.*

Through the points H and T describe a circle cutting HF, TF produced in x and y, then the angle DTF = DHT = xyF; consequently xy is parallel to DT. If therefore xH be any chord in a circle, and yT another, cutting the former in F, xy being joined, from T draw TD parallel to xy to meet xH in D, then the 16 rectangle HFD = FT². *Which is the sixteenth Proposition.*

Prop. XVIII. Let ABC be a triangle inscribed in a circle, whose sides AB and AC are equal, and from A any line be drawn meeting the circle again in D and BC in E; I say that the rectangle DAE is equal to the square of AB.

Prop. XIX. Things remaining as in the last proposition, if lines touching the circle in A and C be drawn to meet in F, and FD be drawn cutting BC in G; I say that the rectangle BCG is equal to the square of CE.

Prop. XX. Let ABC be a triangle inscribed in a circle whose sides AB and AC are equal, and let AD be parallel to BC, and taking any point therein D, let the rectangle under AD and P be equal to the square of AB or AC, and from the points A and D let the lines AE, DE be inflected to any point E in the circle, meeting BC in F and G; I say the rectangle under FG and P = the rectangle BFC.

Join TH, T′H and from T′ set off T′B so that TT'^2 = the rectangle HT′B, and let TE be any line cutting T′H in A, and the circle again E′ and from E′ let a parallel to TT′ be drawn to meet T′H in γ, then (by the last) $AE'^2 = A\gamma \cdot AH$ and the △s AγE′, AHE′ and consequently AT′T similar, as also E′Hγ, T′HE′, therefore as $AE'^2 = A\gamma \cdot AH$: AT'^2 : : $E'H^2$: TT'^2; but γH : E′H : : E′H : T′H, ∴ $E'H^2$ = γH . TH : TT'^2 = T′B . T′H : : γH : T′B; consequently HA . Aγ : AT'^2 : : γH : T′B. *Which is the seventeenth Proposition.* 17

Let Hh cut TT′ in i, then GH : TH : : TH : DH and GH : iH : : hH : DH;

Prop. XXI. If in AB the diameter of a circle be taken two points C and D such that AC : CB : : AD : DB, and D be within the circle, and DE be perpendicular to AB meeting the circle in E and F, and if through C any line be drawn meeting the circle in G and H, and the line DE in K, and GL touch the circle in G, and meet DE in L; then I say the rectangle LDK is equal to the square of DE.

Prop. XXII. If in AB the diameter of a circle be taken two points C and D such that AC : CB : : AD : DB, and D be without the circle, and DE be perpendicular to AB, and through C be drawn any line meeting the circle in G and H, and the line DE in K, and GL touch the circle in G, and meet DE in L; then I say the rectangle LDK is equal to the rectangle ADB.

$\lambda nE = \lambda GE = GEE' = GE'E = \frac{1}{2}hnE$, therefore λn bisects the angle hnE, and because $nh = nE$, therefore $ht = tE$ is perpendicular to $n\lambda$, therefore the same circle passes through h, and $h\lambda = E\lambda$ is perpendicular to hn, and $Gg \cdot g^{\lambda} = hg \cdot gE = Tg \cdot gT'$, add Gg^2 and $Gg \cdot G^{\lambda} = Tg \cdot gT'$
21 $+ Gg^2 = TG^2$. *Which is Proposition* 21*st. both cases.*

Diagram III.

Through h draw EK cutting the perpendicular through F in K, and produce E^{λ} till it cuts it in l, then the triangles

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ing the circumference in D and N, in CD let be taken two points E and F on the same side of C with D such that the rectangle ECF may be equal to the square of CD, and from the points E and F let EG, FG be inflected to any point G in the circumference, meeting the same in H and K, and let HK when drawn meet the diameter AB in L; then I say that AL : LB : : AC : CB.

Prop. XXVIII. In AB the diameter of a circle produced let be taken the point C, and CD be perpendicular to AB, and therein be taken two points E and F on different sides of C such that the rectangle ECF, may be equal to the rectangle ACB, and from the points E and F let EG, FG be inflected to any point G in the circle, meeting the same in H and K, and let HK when drawn meet

Eh′E, Fh′K, Gh′g′ are similar as also Ghg, and the points G, h, λ, E are in a circle, the triangle λgE, and consequently hFE is similar to hgG, and consequently to Fh′K, therefore FK : Fh′ : : FE : Fh and FK . Fh = Fh′ . FE = FD . FH. *Which is the twenty-second Proposition.* 22

Produce hD to γ, then by similar triangles FD : Fγ : : Fh : FH. *Which is the twenty-third Proposition.* 23

From h and E to any point N in the circle, let lines be inflected cutting Gλ in k and L, then because the angle ENh = EEh = LGh the points h, L, N, G are in a circle, consequently Lk . kG = kh .

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the diameter AB in L; then I say that AL : LB : : AC : CB.

Prop. XXIX. Let AB touch a circle in B, and any line AE be drawn equal to AB, and likewise from A let any line be drawn to cut the circle in C and D, and let EC, ED be drawn meeting the circle again in F and G; then FG being drawn will be parallel to AB.

Prop. XXX. Let AB touch a circle in B, and therein be taken two points E and F on the same side of A such that the rectangle EAF may be equal to the square of AB, and from A let any line be drawn meeting the circle in C and D, and EC, FD be drawn meeting the circle again in G and H; then GH being drawn will be parallel to AB.

Prop. XXXI. Let AB touch a circle in B, and any

$kN = Tk \cdot kT' = TG^2 - Gk^2$; therefore

24 $TG^2 = Gk^2 + Lk \cdot Gk = LG \cdot Gk$.

Which is the twenty-fourth Proposition.

Since (by the first) Fn : FH :: FD : FG :: Fn : Hn :: FD : DG :: Fn — FD :

25 Hn — DG or Fn : Hn :: Hn : Gn. *Which is the twenty-fifth Proposition.*

Produce ET, ET' till they cut the perpendicular in Δ and Γ; then the $\angle TF\Delta = \Gamma FT'$ and the angle ΔTF made with the tangent is equal to the angle TT'E in the segment $= \Delta\Gamma E$, therefore the triangles ΔTF, $F\Gamma T'$ are similar, therefore $F\Gamma$: $FT' = FT :: FT : F\Delta$, and $F\Gamma \cdot F\Delta$

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line AE be drawn and therein be taken two points E and F on the same side of A such that the rectangle EAF may be equal to the square of AB, and from A any line be drawn to meet the circle in C and D, and EC, FD be drawn meeting the circle again in G and H; GH being drawn will be parallel to AE.

Prop. XXXII. Through any point A within a circle let a line be drawn meeting it in B and E, and therein two points F and G be taken such on different sides of A that the rectangle FAG may be equal to the rectangle BAE, and through A any line be drawn meeting the circle in C and D, and FC, GD being drawn to meet the circle again in H and K; then HK being drawn will be parallel to AB.

Prop. XXXIII. Let AB be a line without a circle,

= FD . FH. *Which is the twenty-sixth* 26
Proposition.

The 28th is the converse of this, viz. if
FΓ . FΔ = FD . FH then DG : GH : : 27
FD : FH. And the 27th of the 24th. 28

If Fa = FT and aE, ah be drawn cutting the circle in p and o, then since Fh : Fa : : Fa : FE, the triangles Fha, FaE are equiangular, therefore the ∠Fha = FaE, also ao : ap : : aE : ah; therefore the triangles aop, ahE are equiangular, ∠ apo = ahE; ∴ opE = Fha = FaE; ∴ op is parallel to aF. *Which is Proposi-* 29
tion 29th.

The 30th is very evident from the 30
printed figure, for since AE . AF = AB^2 = AC . AD, the points C, D, E, F are in a circle, therefore the external angle ACE = EFD and = DHG; ∴ EFD being =

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and from A and B two lines be drawn to touch the circle in C and D, and let the square of AB be equal to the sum of the squares of AC and BD, and from A any line be drawn to meet the circle in E and F, and BE, BF be drawn meeting the circle again in G and H; the points A, G, H, will be in a right line.

Prop. XXXIV. Let AB meet a circle in C and D, and A be without and B within the same, and let the rectangle CAD be equal to the square of AB together with the rectangle CBD, and through A any line be drawn meeting the

DHG, GH is parallel to AB. Take Fl′ = Fl, then Fl′ . FK = FT² = FD . FH = Fq . FN, two lines FN′, KN′ being inflected to any point N′ in the circle cutting it in q and T′, draw l′q cutting the circle again in T, then the points l′ K q N′ are in a circle because Fl′ . FK = Fq . FN′, therefore the angle Fl′q = FN′K = q TT′. consequently TT′ is parallel to FK. Hence if ET touch a circle in T, and any line l′F be drawn, and therein be taken two points l′ and K on the same side of F such that the rectangle l′FK may be equal to the square of FT, and from F any line be drawn to meet the circle in q and N′, and l′q, KN′ be drawn meeting the circle again in T and T′, TT′ being
31 drawn will be parallel to l′F. *Which is Proposition the* 31*st.*

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circle in E and F, and BE, BF be drawn meeting the circle again in G and H; then the points A, G, H, are in a right line.

Prop. XXXV. From the extremes of AB let two lines AC, BD be drawn to touch a circle in C and D, and in AB let a point E be taken on the same side of A with B such that the rectangle BAE may be equal to the square of AC, and also in AB another point F on the same side of B with E such that the rectangle EBF may be equal to the square of BD, and through A any line be drawn meeting the

(The 32d is evident from the printed figure, because FA . AG = DA . AC, F, D, G, C are in a circle; therefore ∠FCA = DGA = DKH, and KH parallel to BG.) If R be taken so that RK . Kl′ = the square of a tangent to the circle from K, then R, l′, T′, N′ are in a circle; through K draw Kq cutting the circle again in s, then R, l′, q, s are in a circle, and ∠l′qK = l′Rs, but l′qK = sqT = TT′s, therefore l′Rs = TT′s and R, s, T′ are in a right line. Hence if from the extremes of FK two lines be drawn to touch a circle, and in FK let a point l′ be taken on the same side of F with K such that the rectangle KFl′ may be equal to the square of FT the tangent from F, and also in FK another point R on the same side of K with l′, such that the rectangle l′KR may be equal 32

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circle in G and H, and BG, BH be drawn meeting the circle again in K and L; then the points L, K, F, are in a right line.

Prop. XXXVI. If from A the vertex of a triangle ABC be drawn AD to any point D in the base, and DE be drawn parallel to AC, and DF to AB; I say the sum of the rectangles BAE, CAF will be equal to the square of AD together with the rectangle BDC.

Prop. XXXVII. Let A and B be two points in the diameter of a circle whose centre is C, and let the

to the square of a tangent from R to the circle, and through F any line be drawn meeting the circle in q and N′, and Kq, KN be drawn meeting the circle again in s and T′, then the points s, T′, R, are in a right line. *Which is the thirty-fifth Proposition.*

Diagram IV.

In the preceding Diagrams it is shewn that ϑH . ϑh which is the square of a tangent from ϑ to the circle is = $F\vartheta^2$ + FD . FH (Prop. 11.) = $F\vartheta^2$ + Fϑ . Fγ (Prop. 23.) = Fϑ × $\vartheta\gamma$; also $\gamma\vartheta$. γF = γh . γD (because in the preceding Diagrams, F, ϑ, h, D are in a circle) = the square of a tangent from γ to the circle; therefore $\vartheta\gamma$ × (Fϑ + Fγ) = $\vartheta\gamma^2$ = the sum of their squares.

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rectangle ACB be equal to the square of the semidiameter; bisect AB in D, and raise the perpendicular DM; from the point A draw AF to any point F in the circumference, and FE perpendicular to DM; then I say that the square of AF is equal to twice the rectangle contained by AC and FE.

Prop. XXXVIII. If any regular figure be circumscribed about a circle, and from any point within the figure there be drawn perpendiculars to all the sides of the figure; the sum of the perpendiculars will be equal to the mul-

Hence we have a ready way of finding two such points ϑ, γ in the perpendicular that $ϑγ^2$ may = the sum of the squares of the tangents from these points to the circle. Let ϑ, γ in Diagram 4, be two such points, and from γ, let a line be drawn meeting the circle in any two points E′ and H′, then ϑH′, ϑE′ being drawn meeting the circle again in h and D′, because $ϑH' . ϑh = ϑF' . ϑγ = ϑD' . ϑE'$; therefore F, h, H′, γ are in a circle, as also F, D′, E′, γ; therefore the $\angle FγH = ϑD'F = ϑhF$, therefore ϑ, h, D′, F are in a circle, and $\angle ϑD'h = ϑFh = ϑH'γ$, also $FD'γ = Fϑh$; consequently the three angles ϑD′h, ϑD′F, γD′F at the point D′ being respectively equal to ϑFh, ϑhF, Fϑh, those of the triangle Fϑh, their sum must be equal to two right angles, and

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tiple of the semidiameter of the circle by the number of the sides of the figure.

Prop. XXXIX. Let there be any number of right lines intersecting in a point, and making all the angles about the point equal, and let any circle pass through the same point; I say the circumference thereof will be divided by the intersecting lines into as many equal parts as there are lines.

Prop. XL. If there be two triangles ABC, DEF, which have one angle A in one equal to one angle D in the

consequently γ, D′, h are in a right line. *Which is the thirty-third Proposition.*

Also because $\gamma D'h = \gamma Fh = \gamma D'E' = \gamma FE'$; therefore Fh, FE′ make equal angles with $\gamma\gamma$ and EE′ is perpendicular to the diameter DH, and parallel to TT′; draw FD′ cutting TT′ in G′, and the circle in N, then $FD' \cdot FN = FT^2 = FG^2 + GT^2 = FG^2 + GG'^2 + TG' \cdot G'T' = FG'^2 + D'G' \cdot G'N$. Therefore TT′ is the locus of all points G′ dividing lines intercepted between F and the periphery so that $FD' \cdot FN = G'F^2 + D'G' \cdot G'N$. From E and h through G′ draw lines meeting the circle again in S and R, and describe as in the 2d Diagram, the circle FTnT′ cutting FN in l. Then because $FG' \cdot G'l = TG' \cdot G'T'$, and this $= SG' \cdot G'E = hG' \cdot G'R$, therefore the points F, l, S, E are in a

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other, and another angle B in the first equal to the sum of the angles D and E in the second; then shall the sides AC, BC, DE, EF be proportional.

Prop. XLI. The square of the line bisecting the vertical angle of any triangle is a mean proportional between the differences of the squares of each side including that angle, and the square of the adjacent segment of the base made thereby.

Prop. XLII. If from the same point two tangents be drawn to a circle, and a line be drawn joining the points

circle, and the angles FlE = FSE. Also D′G′ . G′N + FG′² = (TG′ . G′T′) G′l . FG′ + FG′² = FG′ . Fl = FD′ . FN = Fh . FE, therefore h, G′, l, E are in a circle, consequently the angle EhR = ESR, and the sum of the angles EhR, FlE is equal to two right ones, therefore the sum of their equals ESR, ESF must be equal to two right, and F, S, R in a right line. 34 *Which is the thirty-fourth Proposition.*

Diagram V.

To any point g′ in TT′ from the center n drawn g′n, perpendicular to which thro' g′ draw ba cutting the two tangents in a and b; then the angles at T and g′ being right, the points g′, a, T, n are in a circle, and the angle nag′ = nTg′ = nT′g′; again

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of contact, and another line to be intercepted between the tangents cut the foregoing which joins the point of contact, so as to be bisected in the point of intersection; then I say that the part of that line which is a chord of the circle will also be bisected by the same point.

And, conversely, if the chord cutting the line joining the points of contact be bisected by the point of intersection; then the continuation of the same to meet the tangents will also be bisected by the same point.

Prop. XLIII. If from one of the equal angles of an

the angles at g′ and T′ being right, the points g′, T′, b, n are in a circle, and the ∠nbg′ = nT′g′, consequently nbg′ = nag′ and ng′ bisects both ab and the chord of
42 the circle. *Which is the forty-second Proposition.*

Let fall TC perpendicular to T′n then by similar triangles T′G = ½TT′ : T′n : :
43 T′C : TT′, therefore T′n . T′C = ½TT′². *Which is the forty-third Proposition.*

If any diameter AB be drawn to this circle, and TA, T′B be drawn intersecting in O, through which drawing OF intersecting AB in P and producing BT, AT′ till they intersect in H; then because BA is a diameter, the angles at T and T′ are right, and the points T, O, T′, H in a circle; ∴ the ∠THT′ = TOB = the complement of TBO; but because FT is

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isosceles triangle a perpendicular be drawn to the opposite side; then I say that the rectangle contained under that side and it's segment intercepted by the perpendicular and the base is equal to half the square of the base.

Prop. XLIV. If in a line AB two points C and D be taken; then I say that

$\overline{AB + AD} \times BC + BC^2 = 2ABC + BCD.$

And moreover that

A C D B

$\overline{AB + AD} \times CD + CD^2 = 2ADC + BCD.$

a tangent, the ∠TBO in the segment is = FTG, consequently THT′ = TFG = T′FG, therefore FT′ being = FT, and the angle TFT′ double THT′ F must be the centre of the circle TOT′H, consequently the diameter HO passes through F. Also since the ∠OHT′ = OTT′ = OBA = the complement of PAH, the ∠HPA is right. *Which is the forty-ninth Proposition.* 49

Moreover FE′² : FT′² = FE′.Fh′ : : FE′ : Fh′ : : (by Prop. 15.) E′g′ : g′h′, therefore FE′² : FT′² : : E′g′ : g′h′. *Which is the fifty-sixth Proposition.* 56

Diagram VI.

Having described a circle about the triangle BAC, and produced AD till it cuts in G, draw DF, ED parallel to AB,

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Prop. XLV. If from the vertical angle of any triangle two lines be drawn to make equal angles with the sides containing it, and to cut the base; then I say that the square of one side is to the square of the other side as the rectangle under the segments of the base contiguous to the first side is to the rectangle under the segments contiguous to the other side.

Prop. XLVI. If in AB the diameter of a semicircle any point C be taken, and from thence any line as CD

AC respectively, and then another circle through G, F, C cutting AG in L, and join LF, GC. Then the $\angle AFL = AGC$, $ALF = ACG$ = the supplement of ABG, therefore $ABG = DLF$, and $ADF = BAG$ by construction; consequently the triangles DLF, ABG are similar, and $AG : BA :: DF = AE : DL$ and $AG : AC :: AF : AL$; but $DL + AL = AD$, therefore $BA \cdot AE + CA \cdot AF = AG \cdot (DL + AL) = AG \cdot AD = GD \cdot$
36 $AD + AD^2 = BD \cdot DC + AD^2$. *Which is Proposition* 36.

Diagram VII.

The construction being as in the Proposition, through B draw BF, join FC, make EH = EF, and join AH; then by hypothesis AC : FC : : FC : BC, there-

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drawn to meet the circumference in D, and a perpendicular DE be demitted; then I say that the square of the line AC is equal to the square of the line CD together with the rectangle under the sum of the distances of C from A and C from B and the line AE, when C is taken in the diameter AC produced; but equal to the square of CD together with the rectangle under the difference of the distances of C from A and C from B and the same line AE, when C is taken in the diameter itself.

fore the ∠FBC = AFC, CFB = FAC = HFA, but the ∠AHF = BFH = FBC = AFC, therefore HAF = FCA, and the triangles HFA, FCA similar, consequently HF = 2EF : AF : : AF : AC. 37

Which is the thirty-seventh Proposition.

Moreover, the two triangles AFC, AFB have the angle A common, and the angle AFC = FBC = A + AFB, and taking FI = BF, the angle AIF = FBC = AFC, therefore AC : FC : : AF : FI = FB : : (drawing GK parallel to FB) AG : GK. 40

Which is Proposition 40.

As to the two intermediate Propositions viz. the 38th and 39th; since the double area of any regular polygon is = the continual product of the radius of the inscribed circle, the number of sides and the length of one side; and if it be divided into triangles equinumerous with

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Prop. XLVII. If from one angle A of a rectangle ABCD a line be drawn to cut the two opposite sides BC, DC, the former in F, and the latter produce in E; then I say that the rectangle EAF is equal to the sum of the rectangles EDC, CBF.

Prop. XLVIII. If a rectangle be inscribed in a right-angled triangle, so that one of its angles coincide with the angle of the triangle; then I say that the rectangle under the segments of the hypothenuse is equal to the sum of

the sides, the sum of their double Areas, must be equal to the product of the sum of their perpendiculars and the side of the polygon. Therefore the radius × number of sides = the sum of the perpen-
38 diculars. *Which is Proposition* 38.

And since equal arches of the same circle subtend equal angles as well at the
39 circumference as centre, therefore the 39th. Proposition is manifest.

A C D B.

Because AD + DB = AB, therefore AD + BC = CD + AB, and AB + AD + BC = 2AB + CD, consequently (AB + AD) . CB + CB^2 = 2ABC + BCD. Also AB + CD + AD = 2AD + BC, therefore (AB + AD) . CD + CD^2 =
44 2ADC + BCD. *Which is Proposition* 44.

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the rectangles under the segments of the sides about the right angle made by this inscription.

Prop. XLIX. If from the same point C two tangents be drawn to a semicircle whose diameter is AB, and if the extremes of the diameter and the points of contact be joined, either cross-ways by two lines intersecting in F, or other-ways by two lines intersecting in H; then I say that CF or HC produced to meet the diameter AB will be perpendicular to the same.

Again if AB : AD : : AD : DB, multiplying by AB, AB^2 : AB . AD : : AB , AD : AB . DB. *Which is Proposition* 51. 51

Also AB^2 : AD^2 : : AB : DB, and by composition $AB^2 + AD^2 : AD^2 = AB . DB : : AB + DB : DB$, and multiplying the consequents AB . DB and DB by AD, and dividing them by DB, we have $AB^2 + AD^2$: AB . AD : : AB + DB : AD. 52 *Which is Proposition* 52. For it is evident that this holds whether AB be equal to AD + DB or not.

Diagram VIII.

The rectangle under the difference of two lines or quantities, and the difference of two other lines or quantities is easily shewn to be = the sum of the rectangles

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Prop. L. If in a semicircle whose diameter is AB the chord of 60°. equal to the radius be inscribed and from the center E a perpendicular drawn thereto and produced to meet the circumference in F; then I say that AF, EF, BF are continual proportionals.

Prop. LI. If a line be cut in extreme and mean proportion; then I say that the square of the whole, the rectangle under the whole and the greater segment, and the

under the two greater and two less, minus the sum of the rectangles under each of the greater and each of the less. Therefore $(AC^2-AF^2)\times(CB^2-FB^2)=AC^2.CB^2+AF^2.FB^2-AC^2.FB^2-CB^2.AF^2$. But CF bisects the angle ACB, consequently $CF^2=AC.CB-AF.FB$, $AC.FB=AF.CB$, and $AC^2.FB^2+CB^2.AF^2=2AC^2.FB^2=2AC.FB.AF.CB$; therefore the quantity above $=AC^2.CB^2+AF^2.FB^2-2AC.CB.AF.FB=$ the square of $AC.CB-AF.FB$, and consequently $=CF^2\times CF^2$; therefore $AC^2-AF^2:CF^2::CF^2:CB^2-FB^2$. *Which is Proposition* 41.

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If upon CF produced be taken E so that BE = BF, then the angle $BEF=BFE=AFC$, and the angles at C being equal, the two triangles ACF, BCE are

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rectangle under the whole and the lesser segments are continual proportionals.

Prop. LII. If three lines are continual proportionals: the sum of the squares of the mean and the greater extreme is to the rectangle contained under the same, as the sum of the extremes is to the mean.

Prop. LIII. In every right-angled triangle, as the hypothenuse is to the sum of the sides about the right

similar, consequently EB = FB : CE : : AF : CF. *Which is Proposition* 58. 58

Having taken CFL = CBF, it will be CB : CF : : CF : CL, and the angle CLF being = CFB, ALF must be = AFC, and the triangles ALF, AFC similar; therefore AC : AF : : AF : AL. *Which is Proposition* 59. 59

If the circumscribing circle be drawn about the triangle ACB, and HL′ be drawn parallel to AB to cut it in H and L′, and CH, CL′ be joined cutting the base in M and N, and making equal angles ACH, BCL′ with the sides; then the rectangle AMB = CMH, and CNL′ = ANB, but CM : CN : : MH : NL′; therefore $CM^2 : CN^2$: : CM . MH : CN . NL′ : : AMB : ANB. *Which is Proposition* 45. 45

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angle, so is the said sum, to the sum of the hypothenuse and twice the perpendicular from the right angle.

Prop. LIV. If a right line AD be any-ways cut in B, and from thence a perpendicular BE erected equal to a mean proportional between the whole AD and the part AB, and a circle be drawn through the points A, D, E, and from A perpendicular be erected to meet the circumference in F; then I say that AF, AB, BE, AD are four continual proportionals.

Diagram IX.

Since $EC^2 = DC^2 - DE^2$; $\therefore AC^2 = AE^2 + 2AE \cdot EC + EC^2 = AE^2 + AE \cdot EC + AE \cdot BC + AE \cdot EB + EC^2 = AE \cdot AC + AE \cdot BC + DE^2 + EC^2 = DC^2 + AE \cdot (AC + BC)$. Also $AC'^2 = AE^2 + 2AE \times EC' + EC'^2 = AE \cdot AC' + AE \cdot EB - AE \cdot C'B + EC'^2 = AE \times (AC' - C'B) + DE^2 + EC'^2 = DC'^2 + AE \times (AC' - C'B)$. *Which is Proposition the* 46*th.* 46

Diagram X.

Having through the given rectangle ABCD drawn AE meeting DC produced in E, and cutting BC in F, let fall BL perpendicular thereto. Then by similar

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Prop. LV. In every right-angled triangle, as the difference between the hypothenuse and one side is to the difference between the same side and its adjacent segment, so is the same side to the same segment.

Prop. LVI. If HC be a tangent to a circle meeting the diameter DB produced in H, and from the point of contact C a perpendicular CK to that diameter be drawn, and likewise a line from H cutting the circle in F and G, and the perpendicular CK in I, and F be the nearest point to H;

triangles EA : ED : : AB = DC ; AL, and EA : AD = BC : : BF : LF; consequently the rectangle EDC + rectangle CBF = EA × (AL + LF) = EA . AF. 47
Which is Proposition 47.

Draw FK parallel to DE, then FE : CE : : KF = CD = AB : AL, and FE : CF : : BF : LF; therefore FE × (AL + LF) = FE × AF = ECD + CFB 48
(DKA). *Which is Proposition* 48.

Also AF : AB + BF : : AB + BF : AL + BL + BL + LF = AF + 2BL. 53
Which is Proposition 53.

And AF — BF : BF — LF : : BF : LF, because AF : BF : : BF : LF. *Pro-* 55
position the 55*th.*

Diagram XI.

If CB = AE = EB = CE, and CF = FB, then the angle FCB = FBC, and

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then I say that the square of HF is to the square of the tangent HC as FI to IG.

Prop. LVII. If one side AC of an equilateral triangle ABC be produced to E so that CE may be equal to AC, and from A a perpendicular to AC raised, and from E a line drawn through the vertex B to meet the perpendicular in D; then I say that BD is equal to the radius of the circle which circumscribes the triangle.

= FAE = AFE, and the triangles CFB, AEF similar, consequently AF : AE = CB : : CB : BF. *Which is Proposition* 50.

If CG be drawn perpendicular to EB and EL parallel to CG cutting AC in L; then because the angle FEB = CAB, AC is parallel to EF, consequently LE = CO = EO = BO. *Which is Proposition the* 57*th.*

Diagram XII.

Erect DC perpendicular to the centre D of the semi-circle ACB; join AC, CB, which produce till it meets a parallel FE to CD in F, that cuts the semi-circle in H and AC in G; then because AE = GE, and FE = EB, and AE . EB = HE^2 = FE . GE, therefore GE : HE : : HE : FE. *Which is Proposition the* 60*th.*

Prop. LVIII. If BD bisect the vertical angle B of a triangle ABC and meet the base in D, and if with either of the other angular points A or C as center and the adjacent segment of the base as radius a circle be described to cut BD again in E; then I say that BE is to BD as that segment used as a radius is to the other.

Prop. LIX. If BD bisect the vertical angle B of the

Upon HE produced as a diameter describe a semi-circle through H and A meeting it in K, then EK : AE = GE : : AE = GE : HE : : HE : FE; take ME = EK, and parallel thereto NG = HE, and OE = NG; then NG : ME : : FE : GE, and by division NG : MO : : FE : FG, and MO : FG : : NG = HE : FE : : GE = NO : HE = NG, or MO : NO : : FG : NG, therefore the triangles FGN, MON are similar, and the angle NMO = NFE, consequently the points E, M, N, F are in a circle. And conversely when these points are in a circle, and FE : NG : NG : GE, take EO = NG, and the triangles NOM, NGF are similar; therefore FG : OM : : NG : NO = GE : : by hypothesis FE : NG, and NG = OE : OM : : FE : FG, and by division NG : ME : : FE : GE, or FE : NG : : GE : ME, consequently FE : NG : : NG :

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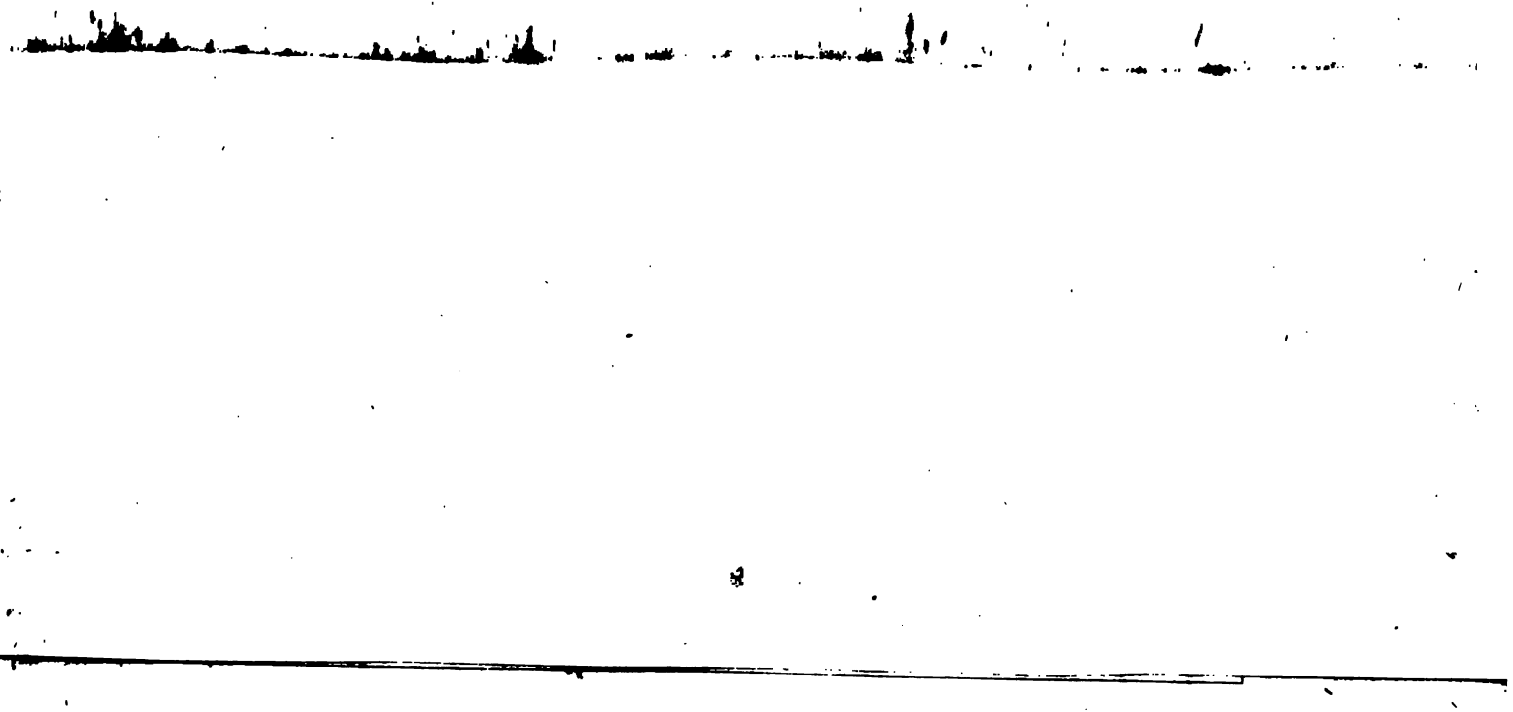
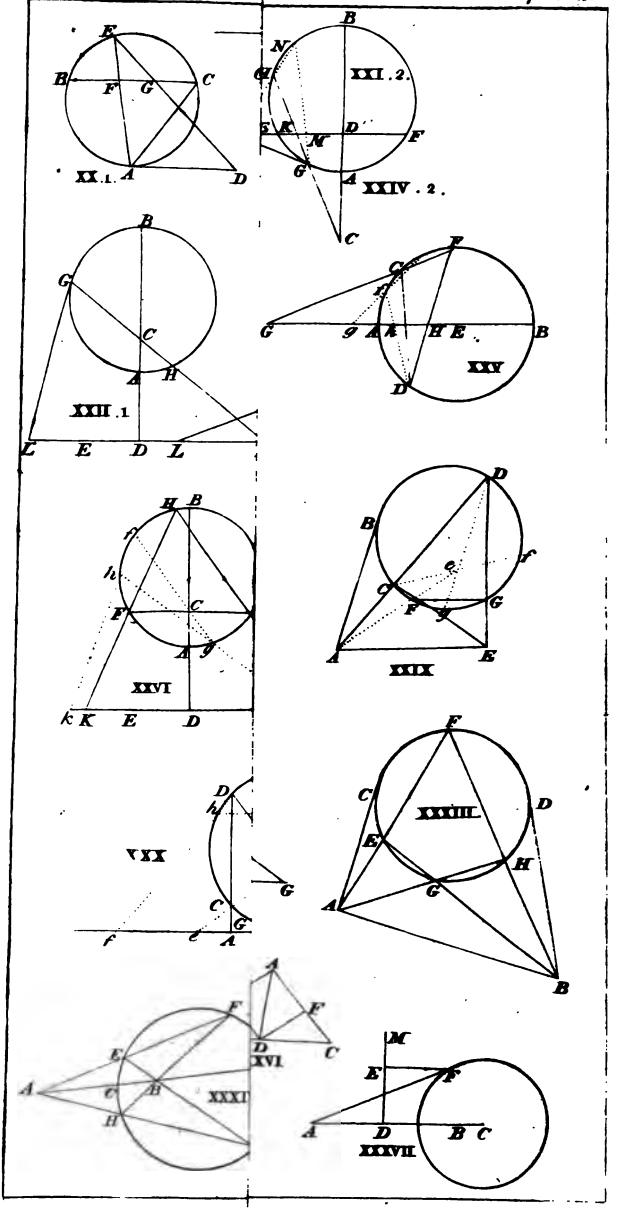
triangle ABC, and if on BA or BC from B be put a third proportional to the other side and the bisecting line; then I say the rectangle under that side on which it is put and its remainder when the third proportional is taken from it is equal to the square of the adjacent segment of the base made by the bisecting line. *i. e.* BCE = CD^2, or BAE = AB^2.

GE : : GE : ME, or ME, GE, NG, FE
54 are four continual proportionals. *Which is the 54th Proposition.*

Its converse is here also proved. And likewise the following, viz. FE being divided by the 60th Proposition, so that GE : EH : : EH : FE, if EL be taken = AE = GE, a circle drawn through the points A, H, L will cut FE produced in K so that EK, GE, HE, FE, are four continual proportionals.

Prop. LX. If an isosceles triangle be inscribed in a semicircle and one of the equal sides produced, and if from any point E in a diameter a perpendicular thereto be drawn to cut the side, the circle, and the side produced in the points G, H and F respectively; then I say that EG, EH and EF are continual proportionals.

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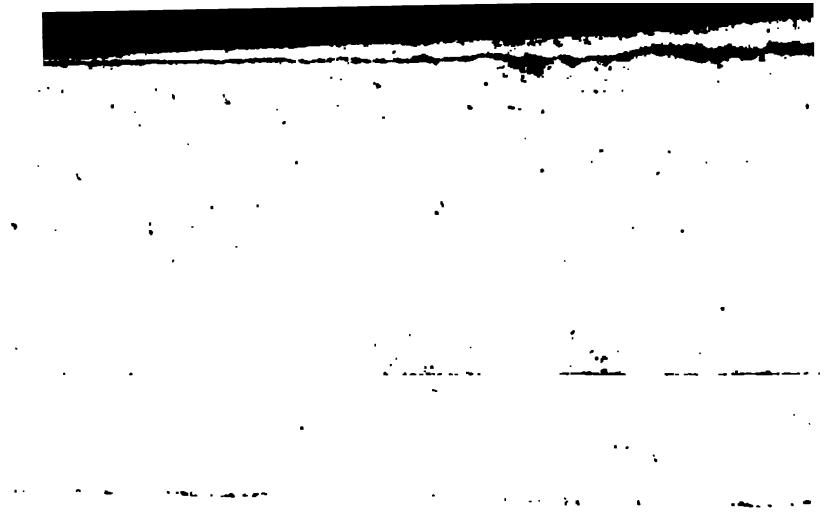

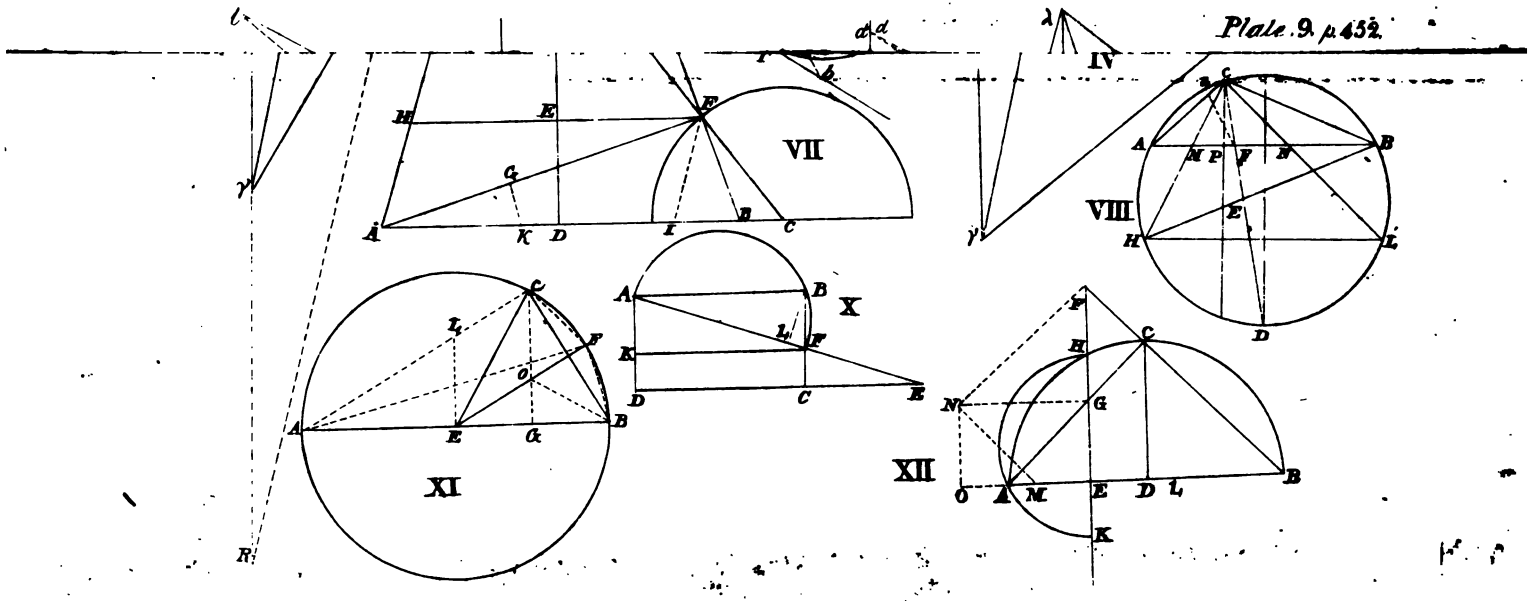
Plate 9. p. 452.
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REMARKS

ON THE

SUMMER BIRDS OF PASSAGE,

AND ON

MIGRATION IN GENERAL.

BY MR. JOHN GOUGH.

COMMUNICATED BY DR. HOLME.

(Read March 20, 1812.)

SIR, Middleshaw, Feb. 21, 1812.

THE following Essay appears to me to contain some new ideas relating to the natural history of periodical birds. Should you entertain the same opinion after perusing the paper, the communication of it to the Literary and Philosophical Society of Manchester, will oblige,

Yours, &c.

JOHN GOUGH.

To Dr. Holme.

PERHAPS no phenomenon in the history of animated nature has engaged the attention of men of observation, in all ages and countries

so generally, as the regular appearance of those birds which visit the northern climates in spring, and disappear as regularly at the approach of winter. But though many facts have been collected, relating to the manners of these singular birds, by the industry of naturalists, their history still remains involved in much obscurity and perplexed with difficulties; many of which in my opinion arise from a negligent or an injudicious arrangement of the facts already ascertained. Philosophers have been induced by this oversight, to take partial views of the subject; and to entertain very discordant notions respecting the winter retreat of the birds in question. All parties, however, are unanimous in concluding, that the regularity of their visits in spring is intimately connected with the apparent motion of the sun betwixt the tropics, whose northern declination is increasing at the time of their appearance, and consequently the temperature of the northern hemisphere is also advancing towards the heat of summer in every latitude. The Philosophers, who have undertaken to discuss this curious question in natural history, agree then, in ascribing the alternate appearance and disappearance of the swallow tribe, the cuckoo, the wryneck, and a majority of the British warblers, to the vicissitudes of

temperature, which are annually experienced in this country, in common with all other places at a distance from the equator. But their unanimity ends here; and, at this point, they split into two parties, who view the subject in very different lights. I intend to state the opinions of each in succession, beginning with those philosophers, who appear to me to have the less degree of probability in their favour; or, to speak more properly, whose notions cannot be defended on their own principles, when these are carefully examined.

Pliny is the oldest naturalist that I recollect, who maintains, that the swallow tribe, and many other birds, with whose winter quarters he was unacquainted, retire to caverns at the end of autumn, where they lie in a torpid state until the return of spring. Many moderns have embraced this idea; and they conclude from a familiar analogy, that the sun, after making certain advances towards the north, recalls these sleepers from a lethargic state, to active existence; in the same manner, that he breaks the winter slumbers of the bat, the field-mouse, and the hedge-hog; as well as of various reptiles, and insects inhabiting the temperate, and frigid zones. This idea is captivating on account

of its simplicity; and I, for one, would not refuse to adopt it, if the accuracy of the analogy were but fairly established. But as this appears to be an impossible task, I shall proceed immediately to state my objections to the supposed constitutional connexion of the birds under consideration, and the animals with which they are compared.

Those quadrupeds, reptiles, and insects, which pass the winter in a state of insensibility; may be recalled to sensation and action at pleasure, by the application of a gentle degree of warmth. This constitutional singularity of these animals, has induced philosophers to conclude unanimously, that the return of the sun in spring rouses them from a torpid condition, at a time when the benefits of the season are ready for their enjoyment. There is another circumstance, which gives something more than plausibility to the supposition when it is properly underderstood. For the animals in question take up their winter quarters, some of them in subterranean habitations, a little below the surface of the soil: others lodge in the crevices of walls or rocks; and a few, such as frogs, female toads, and water newts, bury themselves in the mud of shallow ponds. These retreats are all of them but slightly

covered by a thin stratum of earth, or a sheet of water of a moderate depth; in consequence of which, they are warmed in due season by the rays of the sun, after he has entered the northern half of the ecliptic. The preceding assertion, is not a plausible conjecture built upon probabilities; but a fact, which has been determined by experiment; for the Rev. Dr. Hales, in the course of his experimental enquiries into the process of vegetation, discovered that a thermometer, the bulb of which was buried 16 inches below the earth's surface, stood at 25° of his scale in September, at 16° in October, and at 10° in November during a severe frost; from which point it ascended again slowly, and reached 23° in the beginning of April (old style). Now the latter part of September and the whole of October is the season in which the bat, the hedgehog, the shrew, the toad, and the frog are seen but seldom, and finally disappear. The same animals all leave their retreats and are observed abroad again in the time betwixt the vernal equinox and the middle of April; which circumstance makes the preceding theory agree very well with the variations of temperature, that take place in the winter habitations of those ani-

mals, which are actually known to pass the cold season in a torpid condition.

After making the foregoing remarks on torpidity, I come to certain facts, which are far from favouring the supposed analogy of those animals which are known to be lethargic in winter, and our summer visitors of the feathered tribe. Birds of this description are very numerous in this part of the world at the time of their disappearance; from which circumstance it is reasonable to conclude, that if they take up their winter abode near the surface of the earth, they would be frequently found in the cold season; which is the case with bats, field-mice, and hedgehogs. Though discoveries of this kind are mentioned by various authors, the uncommonness of the circumstance obliges the advocates of torpidity to dispose of the periodical birds during winter, in places which are inaccessible to men, such as the vaults of profound caverns or the bottoms of deep lakes. My objections to this opinion, are derived from certain facts respecting the temperature of places situated at great depths below the surface of the land and water.

Every place on the globe has an invariable temperature peculiar to itself, which cannot be

found at less than 80 feet below the external soil. Mr. Boyle kept a thermometer for a year, in a cave which was situate under a roof of earth 80 feet in thickness; and found, that the liquor in the instrument remained stationary all the time. In compliance with my request, the late Dr. Withering made a similar experiment on a well 84 feet deep, at Edgbaston near Birmingham, the temperature of which was found to be 49° in every month of the year 1798. Pits or wells of a less depth give more or less annual variation of temperature, according to the distance to which they penetrate the superficial strata of the earth. A remarkable singularity, however, is observable in experiments made on pits of a moderate depth. I kept a monthly account of the temperature of a well, for the Years 1795 and 1798, the perpendicular depth of which was 20 feet; and the annual variation of its temperature fell a little short of 4°. But the following circumstance deserves to be carefully remarked on the present occasion. The temperature of the ground, at the distance of 20 feet from the surface, is at the highest in October, when a thermometer exposed to the atmosphere makes the monthly mean coincide with that of the year: on the contrary, the

subterranean temperature does not arrive at a *minimum* before the end of March; which is three months later than the coldest weather above ground.

The facts just stated throw much light on the subject of the present essay, by pointing out the reason which determines animals of known lethargic habits to form their winter retreats near the surface of the ground. This choice exposes them, according to the experiments of Dr. Hales, to a variable temperature, which sinks slowly at first, and keeps them benumbed by a sleepy torpor; but after the rigours of winter are past, the hiding places of these slumberers are gradually warmed by the returning sun, which reanimates their torpid limbs, and recalls them from their secret dens, at the proper moment for their appearance above ground. Had the hedgehog, the field-mouse, &c. made a contrary choice, and retired to caverns 80 feet deep, all the benefit they could have derived from an invariable temperature, would have consisted, in the certainty of not being frozen; for the same degree of cold which disposes them to sleep in autumn, would evidently perpetuate their slumbers in these situations; unless we suppose them to be roused to action by the calls of hunger,

which is a precarious and treacherous cause. For the sense of want would not fail in many instances to invite these animals to certain death in the midst of frost and snow, at an earlier season than the commencement of spring. If we suppose our known sleepers, or any other animals suspected of torpid habits, to retire to a depth less than 80 feet, but to a distance from the surface which is sufficient to conceal them, in damp and dreary grottos, from human observation; the supposition will not remove the difficulty. For the time when our periodical quadrupeds, birds, and reptiles disappear, coincides with the *maximum* of temperature in such places, and they are seen abroad again when the same temperature is at the lowest.

Very few arguments will be now required to demonstrate the impossibility of the analogy which is supposed to connect the periodical birds of summer, and the sleeping animals of winter. It is sufficient barely to remark, that the former are never found slumbering with the latter, near the surface of the earth; and deep caverns are proved to be unfit for the reception of any creature in the torpid season. Consequently the birds in question, desert the

temperate zones at the approach of winter, to seek a better climate in lower latitudes.

The migration of our summer visitors being established upon authentic facts, I intend to proceed in the next place, to give a theory of their annual motions derived from natural causes. All the birds constituting the migrating tribe feed upon insects, which disappear and become torpid, either in a perfect state or under the form of embryos, soon after the autumnal equinox. This circumstance, refuses the animals under consideration a farther supply of proper aliment in the higher latitudes. They are therefore compelled by the apprehension of starving, to use their wings and retire southwards into more genial climates, where the rigours of winter do not lock up the sources of their natural food. The manners of the winter birds of passage favour the last conclusion; for the jack-snipe, the red-wing, the woodcock, and the fieldfare, with some other species, quit the frosty regions of the north at the approach of cold weather, and spend the winter in the more temperate parts of Europe. But the return of spring admonishes them when to leave these countries; and they retire generally before the end of April, to pass the breeding season on the confines of the arctic circle.

The twite (Fringilla montium) breeds on the hills of Yorkshire and Westmoreland, but does not remain all the year in its summer habitation. For twites congregate in multitudes about the beginning of October and disappear; but large flocks of them are seen at that time, or not long after, in the south of England. Thus are the two retreats of this migrating finch pretty well ascertained. But the same cannot be generally affirmed of those birds which retire from Britain in autumn. The swallow, however, is now known to winter in different parts of Africa; and, in all probability, future observers will discover the southern retreats of the other migrating species partly on the same continent, and partly in the warmer countries of Europe or in the corresponding districts of Asia. The last opinion must be received as a conjecture, but it has the recommendation of being probable; because those birds which return hither about the time of the vernal equinox, may be expected to pick up a livelihood near home during the preceding months, without accompanying the swallow to the mouth of the Senegal, in the 16th degree of north latitude. Finally we may conclude, apparently with safety, that no bird retires in autumn farther from its summer residence than necessity

requires; and that its winter abode is fixed by the article of food, which depends on the temperature of the place, and the appetite of the visitor.

After making the foregoing imperfect remarks on the southern retreats of the migrating tribe, I come in course to the cause which invites these wanderers northward, to spend the summer in higher latitudes. No sooner has the sun touched the tropic of Capricorn, than he begins to lessen his southern declination, and to shine more directly upon the opposite hemisphere: every latitude of which experiences his animating influence in succession, commencing with the parts contiguous to the torrid zone, and proceeding gradually to the frozen regions within the arctic circle. The advances of spring towards the north, keep pace with the diffusion of solar heat over the northern half of the globe. For the same plants flower much earlier in low than in high latitudes; and we may safely conclude that the same lethargic animals, especially the same flies and other insects, will observe the like rule in quitting their winter quarters; and will appear abroad in Italy much sooner than in Britain. The following comparative facts may serve to elucidate the slow progress of spring from the

south to the north. I am sorry, that the observations are chiefly confined to the vegetable kingdom. The table, however, contains a remark, which is of importance to the present subject. For it traces the nightingale, a feeble bird of passage, through 22° of north latitude; by assigning the times of its appearance on three distant parallels. Now it has been shewn, that the periodic birds do not remain torpid through winter, in those countries which they frequent in summer; consequently, we may infer with safety, that the nightingale travels leisurely towards the arctic circle during the vernal months, after leaving its winter retreat which is unknown. In this long journey, this bird passes from one degree of latitude to another, as the advances of spring prepare the successive climates of the northern hemisphere for its reception, by warming the ground, and calling the insects of each country progressively into active existence.

The Progress of Spring shewn by the Time of flowering of the same Plants in different Latitudes.

Name.	Athens, lat. 37° 25′	Stralton, lat. 52° 45′	Kendal, lat. 54° 26′	Upsal, lat. 59° 36′
Leucoium vernum. f.	Feb. 1	——	——	April 13
Narcissus pseudo-Narcissus. f.	Feb. 5	——	March 22	May 15
Anemone nemorosa. f.	Feb. 14	April 10	——	——
Ulmus campestris. v.	Feb. 16	April 10	——	May 15
Cratægus Oxyacantha. v.	March 2	——	April 15	May 15
Nightingale sings.	March 24	April 22	——	May 15

This deliberate manner of travelling relieves the theory of migration from one of its principal difficulties. For this supposition makes an easy task of a long journey to those birds of passage which are not remarkable for agility and power of wing; such as the redstart, the yellowwren, the nightingale, and other species. These wandering birds are not required by the theory, to fly with the greatest expedition through 40 or 50 degrees of latitude, from their winter quarters to their summer haunts. On the contrary, one of them has been proved to move slowly from one station to another, as the sun advances in his return towards the tropic of Cancer. The winter labours of the jack-snipe, which is remarkable for its inactive habits, confirm the foregoing supposition. For this bird quits the northern regions early in autumn; and, in spite of its natural feebleness and indolence, makes a shift to travel over the greatest part of Europe in the cold season. The woodcock also, after leaving the same summer retreats makes a similar journey, and passes over into Africa.

I shall now proceed to give a few points in the vernal course of the chimney swallow *(Hirundo rustica,)* which is known to travel in the spring from Senegal, in latitude 16° north, to Drontheim, in latitude 64°

north. This bird appears in the neighbourhood of Senegal on the 6th of October; and has been seen as late as February in the same country. It is said to arrive at Athens, in lat. 37° 25′, on the 18th of February; at Rome, in lat. 41° 45′, on the 22d of the same month; at Piacenza, in lat. 45°, March 20th, A. D. 1738; at Tzaritzin, in lat. 48° 30′, April 4th; in the late spring of 1793, at Catsfield, lat. 51°, April 14th, from a mean of twenty observations; at Stratton, lat. 52° 45′, April 8th, from a mean of twenty observations; at Kendal, lat. 54° 20′, April 17th, from a mean of twenty-three observations; at Upsal, lat. 59° 80′, May 9th, from one observation.

This route of the swallow towards the arctic circle, shews that the bird does not rely on its agility, and loiter in the torrid zone longer than is necessary. On the contrary, it travels slowly from climate to climate, until the sun is in 17 or 18 degrees of northern declination, and spring has made considerable advances in the ungenial climate of Sweden. One anomaly occurs in the vernal progress of the swallow, which deserves the attention of the naturalist, because the circumstance when properly understood, shews how attentive the bird is to the local causes, which retard the spring in certain districts. The swallow ap-

pears upon an average, six days earlier at Stratton in lat. 52° 45′, than at Catsfield in lat. 51°. There is little or no doubt that this apparent exception to the present theory arises from some circumstances which retard the increase of the vernal temperature at Catsfield; and make the spring advance more quickly at Stratton. As I am unacquainted with the situations of both places, it will be proper to state a few facts, which shew how powerfully causes of this sort influence the excursions of migrating birds. 1st. The bank martin, *(Hirundo riparia)* is commonly seen at the mouth of the river Kent six or seven days before it arrives at Kendal; though the distance does not exceed five or six miles. But the town lies near the mountains; and the air is colder in that part of the valley than at the head of the estuary. 2d. I have frequently heard the redstart, the yellow-wren, and the white-throat singing in the gardens at Kendal, two or three days prior to their arrival at Middleshaw. I attribute this difference to the same cause; for Middleshaw lies 200 feet higher than the town, being distant from it three miles to the south east. Lastly, the chimney-swallow was seen at Kendal on the 24th of April, A. D. 1808; but did not make its appearance at Settle, before the first

of May. The latter town lies south of east thirty miles from the former, in a mountainous district not far from the source of the Ribble.

The preceding instances, with other facts of a similar nature, shew how absolutely the motions of the birds under consideration, are regulated in the vernal months by local causes affecting local temperature; and the principal object of the present Essay may be called an attempt to demonstrate, that the same leading cause, naturally connected with the article of food, compels them to traverse the temperate zone, wholly or in part, twice in the course of the year. When the phænomena of migration are considered in this way, winter and summer birds of passage become relative terms belonging to the place of observation. For instance, the twite inhabits the southern parts of Britain during the cold months, but returns to the hills of Yorkshire in spring; and if we may judge from the opposite climates of the torrid and frigid zones, the former will have no visitors but in winter, and the latter none excepting in summer. The intermediate space on the surface of the globe is the chief scene of their operations. It is here that the temperature of the atmosphere undergoes great variations, but never

arrives at extremes; in consequence of which, every wanderer of the feathered tribe has the power of selecting a summer residence in the temperate zone which is agreeable to its feelings and appetite. The different kinds of these birds can naturally subsist in places where the spring has made less or greater advances; for the redstart precedes the swallow, and the swallow precedes the cuckoo. This is the reason why the different species travel in distinct parties, resembling the legions of a numerous army marching in the same direction; the whole body being in motion together alternately to the north and south. I shall close the Essay with a table exhibiting the order of this procession in Westmoreland. The first column contains the names; the second gives the times of migrating northwards, which is when the winter birds depart, and the summmer visitors arrive; the third gives the times of migrating southwards, that is, when the summer birds depart, and the winter visitors arrive.

TABLE.

Birds.	Migrate		
	North.	South.	
Anas Cygnus.....		Jan. or Feb.	In hard frosts
Fringilla montium.....	March 1	October 4	
Anas Anser	March 8	September 10	
Numenius Arquata.....	March 10	September 9	
Tringa Vanellus.....	March 13		
Motacilla flava	March 21	October 24	
Sylvia Hippolais	March 26		
Motacilla Boarula	April 4		
Scolopax rusticola	April 8	October 14	
Hirundo riparia	April 12		
Turdus pilaris.....	April 14	October 18	
Sylvia Phœnicurus ...	April 14 in exposed situations	October 3	
Sylvia Trochilus.....	April 15		
Hirundo rustica.....	April 17	September 25	
Tringa hypoleucos ...	April 22		
Sylvia Sylviella.....	April 26		
Cuculus canorus.....	April 27		
Hirundo urbica	April 29		
Sylvia rubicola	May 1		
Charadrius Morinellus	May 2		A. D. 1793
Sylvia cinerea	May 2		
Hirundo Apus	May 3	August 18	
Sylvia sylvicola.....	May 13		
Sylvia hortensis.....	May 15		
Sylvia salicaria	May 17		

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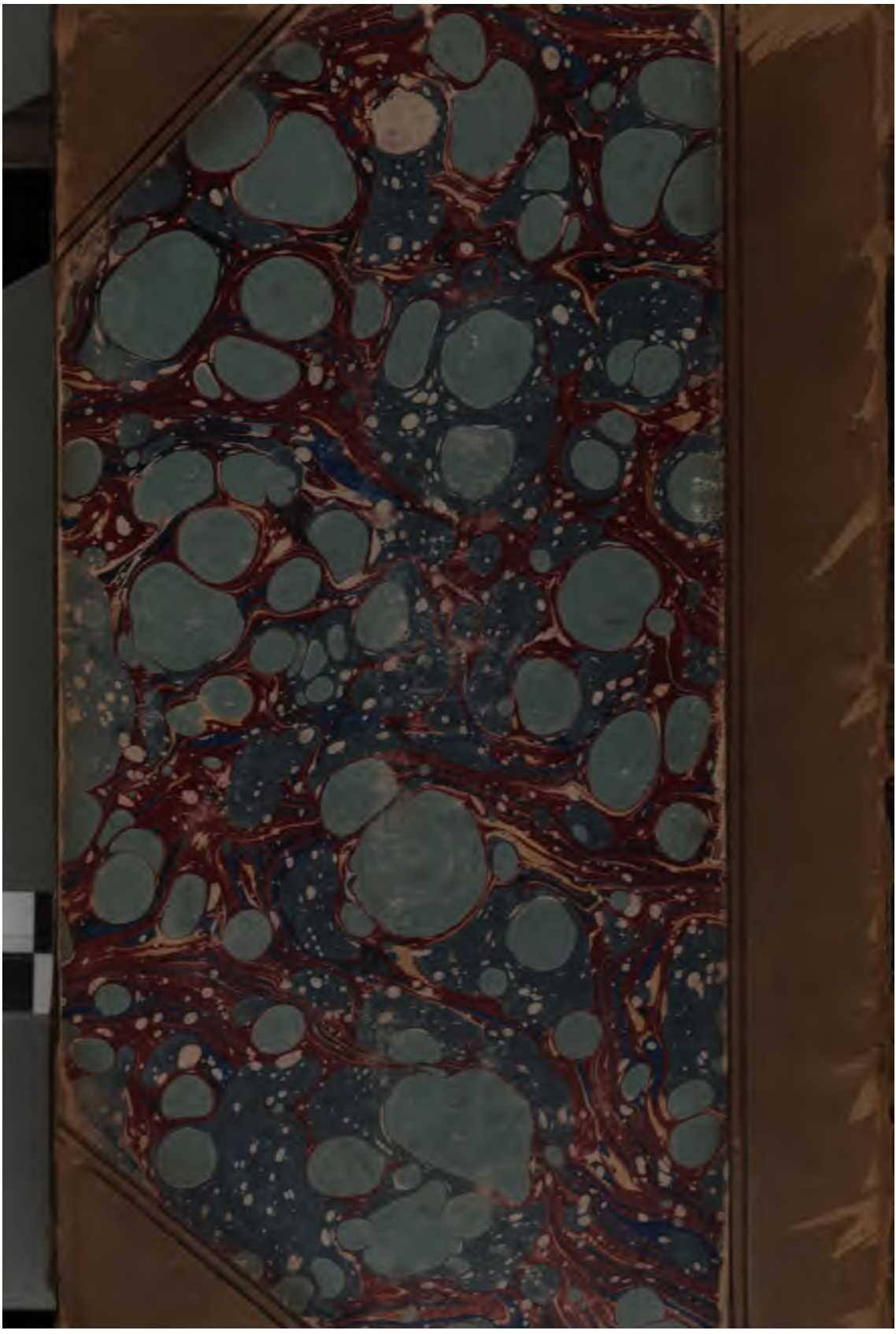
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